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EDGAR RUBIN

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THE PSYCHOLOGICAL REVIEW

EDGAR RUBIN [1]

1886–1951

Edgar Rubin died on May 3, 1951 after a protracted illness which was brought on in part by the hardships which he suffered in his flight to Sweden during the German occupation of Denmark. He was born on September 6, 1886, in Copenhagen, where from 1904–11 he studied philosophy and psychology under Harald Höffding and Alfred Lehmann. From 1911–14, under the direction of Georg Elias Müller in the Institute of Psychology at the University of Göttingen, he carried out his classical investigations of figure and ground. The degree of doctor of philosophy was conferred on him in 1915. The next year he returned to the University of Copenhagen where for two years he was *dozent* and for four years lecturer in philosophy. In 1922 he was appointed to succeed Lehmann as professor of experimental psychology and director of the psychological laboratory. Except for two years of exile in Sweden as visiting professor at the University of Lund, Rubin was actively engaged in teaching and research at Copenhagen until the time of his death. He was president of the Danish Association for Philosophy and Psychology, and also president of the 10th International Congress for Psychology, which he organized in 1932.

Aside from his two Danish teachers, G. E. Müller exerted the most decisive influence on Rubin's basic thinking, although like other experimental psychologists he was deeply impressed by the phenomenological point of view which at that time had pervaded the scientific atmosphere of Göttingen as a consequence of the spell cast by the ideas of Husserl. This outlook became apparent in his chief work, *Visuell wahrgenommene Figuren: Studien in psychologischer Analyse*, which appeared in Danish in 1915. The German edition of this work must be regarded as one of the books which has had a compelling influence on the development of the psychology of perception in recent decades. In this volume Rubin gave evidence of remarkable versatility and thoroughness in dealing with those optic phenomena which result from the articulation of the visual field into organized figures and unorganized background. The findings obtained in these studies and the interpretation which he gave to them brought Rubin into close proximity with Gestalt psychology. As in this major work, so in all later publications Rubin revealed an almost exclusive interest in questions of perception: perceived movement in visual perception, tactual and auditory impressions, and the sensations of temperature as well as gustatory impressions during reception of food. Mention should also be made in this connection of the exact and very penetrating analyses of the temporal sense—a field which unfortunately has been badly neglected by experimental

[1] Translated by the editor from the German.

psychology. In all of these investigations Rubin devoted himself with outstanding success to clearing up the relations between psychology, physics, and epistemology, which nowhere else are so numerous and close at hand as in the domain of perception.

In psychology Rubin stood for the kind of inquiry which, so far as possible, is not encumbered with hypotheses, for the kind of investigation in which the facts are allowed to speak for themselves. In this regard he was somewhat opposed to the efforts, which are by no means confined to *Gestalttheorie,* to turn psychology in the direction taken with such great success by modern physics, that of constructing bold hypotheses. "Psychologists ought not to use theories as a cloak for their ignorance," was one of his frequent assertions. He urged instead a procedure which he himself called "adspektiv." By that term he meant that psychologists should not build up wholes out of elements derived from analyses of mental phenomena along the lines proposed in such doctrines as "mental chemistry," "creative synthesis," and "production-theory." They should attempt rather to get at the data of mind by observing them from those angles that do not distort their intrinsic nature. He tried to clarify this point of view in the stand which he took towards Höffding's concept of synthesis. "Regarding the problems with which I was occupied, the mental synthesis of Höffding seemed to me to be a makeshift introduced in order to build up the wholes which had been broken by the analysis which led to sensations. I considered by not breaking up the wholes and by keeping to them and their aspects in my investigations I could save using this makeshift." "Adspektiv" psychology has no traffic with elements. It prefers to concentrate its research upon the varied aspects which the total pattern presents for inspection.

The collection of papers, selected by Rubin himself and published in 1949 in Copenhagen under the title *Experimenta Psychologica,* gives an excellent insight into his thinking and his methods of inquiry. A thorough study of this volume will furnish much lively stimulation and can be warmly recommended to the experimental psychologist. The sections dealing with *"haptische Untersuchungen"* are especially worthy of note. These studies reveal the workings of an unusually able and critical mind which combined a rare aptitude for certain kinds of psychological problems together with a passionate desire to unravel the problems by every possible experimental device.

Whoever enjoyed the privilege of close association with Rubin as friend and colleague had occasion to admire his extraordinarily quick comprehension of all psychological questions, to note his grasp of another's line of thought before it was fully expressed, and especially to appreciate the critical gift which enabled him to take an argument apart and point out its flaws in such humorous manner that even caustic criticism never gave offense.

DAVID KATZ
University of Stockholm

VICARIOUS TRIAL AND ERROR[1]

BY J. G. TAYLOR AND B. REICHLIN

University of Cape Town

In certain learning situations animals display a form of behavior which Muenzinger (4) has called "vicarious trial and error" or VTE. This is assumed to be a feature of behavior at any point of choice, but it is most readily seen when the response required of the animals involves a fairly rapid expenditure of energy, as in jumping. When a rat is placed on a platform from which it can jump in either of two directions, the rewarded direction in each trial always being indicated by, say, a black card, and the unrewarded direction by a white card; and if the number of the preceding trials has not been sufficient to ensure that the response will be invariably correct, then the animal may go to the edge of the platform, say at the left-hand side, and stretch its head out across the gap in the direction of the left-hand stimulus. It may even lift its front legs and thrust itself farther forward, balancing itself on the edge of the platform as if it were about to jump. But instead of taking off, it may then withdraw, move a little to the right and repeat the same performance in the direction of the right-hand stimulus. There may be several repetitions of this act before the rat finally jumps in one direction or the other. In other cases the stretching and balancing components of the act may be largely eliminated, and all that remains is a moving of the head from side to side, an orientation of the rat in the direction now of one stimulus, now of the other.

Among the most important facts about VTE behavior, as recorded in the literature, the following may be mentioned.

1. The number of VTE movements per trial is low at the beginning of learning. After one or two trials their number increases, rising to a maximum as the proportion of correct choices approaches unity, and then falls to zero during the final stage of the learning.

2. The maximum number of VTE movements per trial is an increasing function of the difference, in j.n.d. units, between the stimuli which are used to mark the right and wrong directions. That is, VTE is more frequent in the case of an easy discrimination than in the case of a difficult discrimination.

3. This relationship is reversed in the case of human subjects who are merely required to discriminate between the two stimuli but to take no further action relative to the chosen one.

4. VTE movements occur more frequently when the stimuli to be discriminated are relatively near to the gap that is to be jumped than when they are farther away.

5. There is more VTE when shock accompanies the correct response and when shock accompanies the wrong response than when there is no shock at all.

To account for such facts Tolman (6) has postulated two sets of intervening variables which, for expository purposes, he has incorporated in a tropistic

[1] The research described in this paper was made possible by grants from (a) The S. A. National Council For Social Research, and (b) The University of Cape Town. We wish to record our warm appreciation of this assistance.

robot called the "Schematic Sowbug." The first of these variables controls the robot's orientation towards the stimuli, and, failing the intervention of the second set of variables, would presumably cause VTE indefinitely. A stimulus striking obliquely on the bug's receptor-organ creates an "orientation readiness" which diminishes as soon as the resulting orientation-response brings the stimulus fully to bear on the receptor. But this has the effect that the second stimulus now strikes the receptor obliquely, thus arousing another orientation-readiness and therefore swinging the robot back to its original position; then the cycle begins again. The difference between the two orientation-readinesses when the bug is directed towards one of the stimuli is conceived to be an increasing function of the difference between the stimuli, so that when these are nearly alike, and therefore hard to discriminate, the orientation-readinesses are also nearly equal and there is in consequence very little VTE. This part of the theory therefore accounts for the higher VTE frequency in the case of an easy discrimination.

The second set of intervening variables, called "progression-readinesses," have certain features in common with Hull's reaction potential ($_sE_R$) in that their magnitudes are determined, although in an unspecified manner, by drive, incentive motivation and the consequences of previous responses to the two stimuli. But there is one important difference. Hull assumes that when there are two competing reaction potentials the reaction is determined by that one which happens at the moment to be the stronger, and the whole of that stronger $_sE_R$ is active in producing the response. Tolman, on the contrary, assumes that the energy available for driving the animal to jump instead of swinging from side to side is proportional to the *difference* between the two progression-readinesses. The weaker progression-readiness cancels an equal quantity of the stronger one.

The difference between the progression-readinesses at any moment depends not only on the differential consequences of jumping in the right and wrong directions but also on the momentary orientation of the bug to the stimuli. If, after repeated rewards for jumping in the right direction and punishments for the wrong direction, it is facing the right stimulus, the difference between the progression-readinesses is positive and the creature moves forward to get another reward. If it is facing the wrong stimulus the difference is negative, or positive but negligible, so that the creature holds back, thereby allowing the reciprocal action of the orientation-readinesses to swing it into the right direction.

This feature of the sowbug's constitution was specifically introduced in order to explain facts 2 and 3 listed above, and it must be admitted that it achieves this purpose. A human subject, required to choose the darker of two nearly equal shades of grey, experiences no world-shaking consequences if his choice should be wrong, and his progression-readinesses both remain at a low level. It follows that when he is looking at the right stimulus the difference between the two progression-readinesses is negligible, and he is free to keep on swinging. But if black and white are substituted for the two greys, then when he is looking at the black, the difference between the progression-readinesses is enough to make him "jump" at once. On the other hand a rat that has bumped its nose against the wrong stimulus in a hard discrimination problem and been rewarded for jumping at the right one acquires substantial pro-

gression-readinesses, but the orientation-readinesses are nearly equal, so that it tends to jump in the direction it happens to be facing. So the man facing an easy problem and the rat facing a hard one both fail to show VTE, but for different reasons: the man because the forward urge comes into action before the orientation-readinesses have had time to deflect him from his course, the rat because there is no force strong enough to deflect him from the first direction he happens to take up.

But in accommodating his robot to fit facts 2 and 3, Tolman has made it impossible for it to act in accordance with facts 1 and 5. Fact 4 he fits into the scheme by the simple expedient of making his bug short-sighted, so that an easy discrimination presented at a distance is equivalent to a hard discrimination at short range. This fits the geometric pattern of the sowbug very neatly, but that hardly justifies the assumption that rats are myopic. Indeed Hebb (1) has presented evidence that distant visual stimuli are more effective than near ones in determining the rat's behavior. The *ad hoc* sensory defect of the sowbug does not add to our understanding of the behavior of real organisms.

With regard to fact 1, the constitution of the sowbug is such that at the beginning of learning the difference between the two progression-readinesses is negligible in comparison with the difference between the two orientation-readinesses when the bug is directed towards one stimulus. There should consequently be more VTE than later when the progression-readinesses have expanded a bit. In fact, Tolman's theory leads to the deduction that the number of VTE movements is a decreasing function of the number of trials. This deduction is not confirmed by experimental data.

At first sight fact 5 seems to agree with the theory. The effect of both rewarding and punishing a correct response should be that the relevant progression-readiness increases rather slowly, and the difference between the two readinesses increases more slowly than when the right response is rewarded and the wrong one punished with shock, or when there is no shock at all. Consequently the orientation-readinesses have virtually undivided control over the organism, and VTE is not likely to be interrupted by a forward-moving tendency. But for the same reason learning should be slow. What ultimately prompts the animal to go forward is the difference between the two progression-readinesses, and if that remains low by reason of the balancing effect of reward and punishment for the correct response, the probability of the wrong response being made must remain high. But it does not. Learning is in fact quite rapid.

This brings us to another aspect of Tolman's theory. He thinks that VTE is an aid to learning. The mechanism of this process is apparently that when the animal is facing in the right direction, the stimulus "reminds" him, so to speak, of the previous satisfactory consequences of responding to it, and that has the effect of increasing the relevant progression-readiness. Similarly, facing in the wrong direction diminishes the other progression-readiness. Hence the more VTE, the more quickly the difference between the progression-readinesses grows, and the more quickly the animal learns. But if facing in the right direction reminds him of both reward and punishment, while facing in the wrong direction reminds him only of previous uneventful but futile excursions, the difference between the two progression-readi-

nesses is going to grow quite slowly. But that is just the reverse of what happens.

The shortcomings of this theory are obvious enough. Indeed Tolman's own explanations of behavior of the kind the bug was invented to account for keep on bursting beyond the bounds of the creature's none too tough cuticle. And the chief reason for this seems to be that the characteristics put into the bug were selected with the specific purpose of explaining VTE. It is theoretically possible to construct a robot that will imitate almost any selected aspect of behavior, and there are probably several ways of constructing any such robot. But our purpose as scientists is not to construct *any* mechanism that will perform the desired trick; it is to discover, if possible, the mechanism that actually does perform the trick in living organisms. And that means that the mechanism must be consistent with other behavioral mechanisms disclosed by research. For this reason we propose to present in the following pages a hypothesis about VTE based on the principles of behavior as summarized by Hull (**2**), since these principles seem to us to be the most consistent and systematic explanation of the behavior of organisms that has yet appeared.

We begin by postulating that a VTE movement is essentially an incomplete or preparatory response which we shall symbolize by ρ. Since ρ commonly involves a smaller expenditure of energy than the full response, R, to which it leads, its threshold may be assumed to be lower than that of R. That is, ${}_sL_\rho < {}_sL_R$. Hence if ${}_sE_R$ is insufficient to evoke R, it may be high enough to evoke ρ. And when ${}_sE_R > {}_sL_R$, ρ will merge with R, so that it cannot be readily detected as a separate component of behavior. In situations where there is a choice between alternative responses, R_1 and R_2, there will be two preparatory responses, ρ_1 and ρ_2, and when the relevant reaction potentials are both below the reaction threshold but above the threshold for ρ, that is, when

$$ {}_sL_\rho < {}_sE_{R_1} < {}_sL_R $$

and

$$ {}_sL_\rho < {}_sE_{R_2} < {}_sL_R $$

then the uncorrelated oscillations to which ${}_sE_{R_1}$ and ${}_sE_{R_2}$ are subject have the effect that the animal sometimes reacts with ρ_1, sometimes with ρ_2, and sometimes with neither. He reveals VTE.

We hold that VTE is not a special form of behavior arising only in a situation involving discrimination, and demanding a special behavior mechanism of its own, but is simply a succession of incomplete or preparatory responses of the same type as the hesitations and false starts that may be seen when animals are learning to respond to a situation which does not call for a choice between alternatives. For this reason we propose to test our hypothesis in the first instance by applying it to a learning process that is not complicated by the element of choice.

Our basic assumptions are (1) that preparatory movements are mediated by the same reaction potential that mediates the complete response, and (2) that the threshold for preparatory movements is lower than that for the complete response. It has been shown by one of us (**5**) that the characteristics of the distribution of reaction velocity (${}_sV_R$), defined as the reciprocal of reaction latency, can be deduced from the expansion of the binomial $(i + j)^k$, where i is the square of the proportion of the oscillation distribution curve above the reaction threshold, and $i + j = 1$. Hence if we can

find some measure of the preparatory responses analogous to ${}_sV_R$, we should be able to predict the way in which the distribution of this measure differs from that of ${}_sV_R$.

An obvious way to measure the velocity of preparatory responses is to multiply the velocity of the complete response, ${}_sV_R$, by the number of preparatory responses, n', preceding the complete response R. But since we assume that ρ is an integral part of R, it follows that the total number of ρ is one more than the number of abortive responses. We must therefore multiply ${}_sV_R$ by $n' + 1$. This is equivalent to taking the reciprocal of the mean interval of time from the beginning of one preparatory response to the beginning of the next.

For three reasons this is not a very satisfactory measure. In the first place, the reciprocal of the mean interval of time between one ρ and the next is not necessarily the same as the mean of the reciprocals of the actual intervals between successive occurrences of ρ. But as we have not in our experiments measured these intervals we must perforce be content with the compromise.

The second source of error arises from the fact that, especially at the beginning of learning, a rat often turns its back on the stimulus and engages in prolonged exploration of features of the apparatus that are irrelevant to the act that is to be learned. During such activity the relevant stimulus ceases to act, and the stimulus-trace may even vanish. That is, the factor which Hull has called stimulus-intensity dynamism (3) drops to zero. Since the duration of this activity is included in ${}_st_R$ it follows that the mean velocity of ρ may be considerably underestimated.

To deal with this source of error we measured not only the reaction latency, ${}_st_R$, but also the total time, t_ρ, when the animal was facing in the direction of the stimulus. It was this latter value that we finally decided to use for the purpose of calculating ${}_sV_\rho$, the velocity of the preparatory responses. The formula we used was:

$$ {}_sV_\rho = \frac{n' + 1}{t_\rho}, $$

where n' is the number of ρ movements preceding the final one which merges into R.

There is a third source of error for which we have been unable to find any satisfactory corrective. We counted as preparatory responses only those movements which were obviously directed towards the stimulus, but it is clear that any movement which did not get as far as that, and was therefore not recorded, should also be counted as a preparatory response. No doubt this error is balanced to some extent by the use of t_ρ instead of ${}_st_R$, for the former measure excludes the time during which the stimulus-trace is active although the stimulus itself is not acting directly on the receptors. That is to say, both the number of recorded responses and the measured time during which they occur are too small, and the estimate of ${}_sV_\rho$ is therefore likely to be not too far removed from its true value.

If we are right in assuming that ρ and R are both mediated by the same reaction potential, ${}_sE_R$, and that they have different thresholds, it follows that the argument presented in the article on reaction latency (5) referred to above is applicable to the distribution of ${}_sV_\rho$ as well as to ${}_sV_R$, but that these distributions will have different constants.

Corollary 1 of the reaction latency article states that the mean ${}_sV_R$ is proportional to the squares of the proportion of the ${}_sO_R$ distribution that

stands above ${}_sL_R$. From this, taken in conjunction with the postulate that ${}_sL_\rho < {}_sL_R$, we deduce that the mean ${}_sV_\rho$ is higher than the mean ${}_sV_R$ in the same trial. For if the threshold of ρ is below that of R, there is a larger proportion of the ${}_sO_R$ distribution above ${}_sL_\rho$ than ${}_sL_R$. This conclusion also follows from our method of calculating ${}_sV_\rho$, so that its empirical confirmation is of no significance. It is stated here merely for the sake of completeness.

Corollary 4 states that the distribution of ${}_sV_R$ is positively skewed at the beginning of learning, symmetrical in the middle and negatively skewed at the end. This depends upon the increase in the proportion of the ${}_sO_R$ curve that is above the threshold as reaction potential increases. When ${}_sE_R$ is just above the threshold, i is very small in comparison with j, so that the expansion of $(i + j)^s$ yields a skew distribution. But at the same time there is a substantially larger proportion of the ${}_sO_R$ curve above ${}_sL_\rho$, and the distribution of ${}_sV_\rho$ is therefore less heavily skewed than that of ${}_sV_R$. It follows further that as ${}_sE_R$ increases the distribution of ${}_sV_\rho$ becomes symmetrical while that of ${}_sV_R$ is still positively skewed, and is negatively skewed when the distribution of ${}_sV_R$ is symmetrical. In short, the distribution of ${}_sV_\rho$ goes through the same transformation as that of ${}_sV_R$ but starts at a later stage.

Corollary 7 states that "as learning progresses the standard deviation of ${}_sV_R$ gradually increases, reaching a maximum when the distribution is symmetrical, and then gradually diminishes." This follows from the fact that the standard deviation of the binomial distribution is $\sqrt{kij}$. Since at the beginning of learning the distribution of ${}_sV_\rho$ is more nearly symmetrical than that of ${}_sV_R$, it follows that its standard deviation is nearer its maximum value. It reaches its maximum earlier and is already declining while that of ${}_sV_R$ is approaching its maximum.

Corollary 8 states that the coefficient of variability of ${}_sV_R$ is a decreasing monotonic function of the number of reinforcements. This deduction obviously applies equally to ${}_sV_\rho$, but we may deduce further that since the initial value of the mean ${}_sV_\rho$ is higher than that of ${}_sV_R$, the initial value of the coefficient of variability is lower, and it remains lower throughout the course of learning.

Corollary 9a states that "the square root of the ratio of the mean ${}_sV_R$ to the maximum ${}_sV_R(\sqrt{V})$ is an increasing ogival function of the number of reinforcements (n), the exact form of this function depending on the relationship between the standard deviation of oscillation (σ_s) and reaction potential (${}_sE_R$)." This deduction applies to ${}_sV_\rho$ as well as ${}_sV_R$, but since the initial value of the mean ${}_sV_\rho$ is a higher proportion of its final (maximum) value than the initial value of the mean ${}_sV_R$ is of its maximum, it follows that the initial level of the ogive is higher in the case of ${}_sV_\rho$. It is, in fact, a truncated ogive.

In addition to the above deductions there are others which are not directly dependent on those stated in the reaction latency article. These concern the number of preparatory responses that may be expected at different stages in the learning process. A diagram will help to make the argument clear. Figure 1 shows the two reaction thresholds, ${}_sL_\rho$ and ${}_sL_R$ and the ${}_sO_R$ curve cutting across these at various stages in the learning process. These curves are shown as positively skewed at the beginning and negatively skewed at the end of learning. There is as yet no experimental evi-

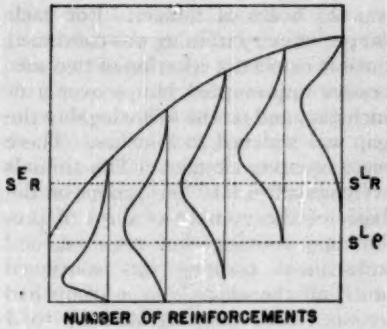

FIG. 1.

dence to justify this, but even if it should prove wrong, the argument that follows is not affected. The curves are also shown as first increasing in range and then decreasing. There is some evidence in support of this (5), but again the general tendency of our argument is not affected.

Consider now the first ${}_sO_R$ curve. A very small proportion of its area is above ${}_sL_R$, and consequently ${}_sV_R$ will be very small. The area above ${}_sL_\rho$ is larger, but it is less than half the total area, so that ${}_sV_\rho$ will also be small. That is to say, there will be long intervals between preparatory responses. Passing to the second curve we find a slight increase in the area above ${}_sL_R$ and a very substantial increase in the area above ${}_sL_\rho$. Hence the mean ${}_sV_R$ has increased slightly, but the mean ${}_sV_\rho$ has increased to a much greater extent. This means that the frequency of preparatory movements per unit of time has increased much more rapidly than the frequency of the complete response.

As we pass from stage 2 to stage 3 the proportion of the area above ${}_sL_R$ increases with positive acceleration whereas the increase in the area above ${}_sL_\rho$ is negatively accelerated. This means that although the frequency of preparatory responses per unit of time continues to increase, the absolute number of such responses is likely to decrease by reason of the shorter length of time that elapses before the process is terminated by the complete response.

By the time we reach stage 4 the entire ${}_sO_R$ curve is above ${}_sL_\rho$, and hence preparatory responses occur with maximum velocity. But now more than half the curve is above ${}_sL_R$, and that means that the time available for preparatory responses is severely curtailed, so that their absolute number drops steeply. At the last stage, when most of the ${}_sO_R$ curve is above ${}_sL_R$, the latency of R has nearly reached its minimum value, and hence ρ nearly always passes without a break into R. That is to say, the absolute number of preparatory responses has fallen practically to zero.

To make these deductions more precise we may assign numerical values to the proportions of the ${}_sO_R$ curve above the two thresholds and calculate the number of preparatory movements that may be expected at each stage.

Table 1 shows, in the first two rows, the proportion of the ${}_sO_R$ curve above

TABLE 1

Stage	1	2	3	4	5
Proportion of ${}_sO_R$ above ${}_sL_R$	.02	.10	.40	.71	.90
above ${}_sL_\rho$	.20	.71	.90	1.00	1.00
Square of proportion above ${}_sL_R$	.004	.01	.16	.50	.81
above ${}_sL_\rho$	.04	.50	.81	1.00	1.00
Number of preparatory movements	100	50	5	2	1.2

the two thresholds. In the next two rows these proportions are squared. Since the velocities of R and ρ are proportional to the values in the 3rd and 4th rows, respectively, the absolute number of preparatory movements is given by dividing the entry in the 4th row by the corresponding entry in the 3rd row. This of course includes the final ρ which merges into R.

These deductions may be summed up in the following two statements. (A) The frequency of preparatory responses per unit of time is an increasing function of the number of reinforcements, rising at first with positive and then with negative acceleration. (B) The absolute number of preparatory responses per trial is a decreasing function of the number of reinforcements.

It should be noted however that in practice the absolute number of preparatory responses in the early stage of learning is likely to be substantially less than theory would lead one to expect. When reaction potential is low, many such responses will be initiated, but will be terminated before their true character becomes obvious to the experimenter. The theory makes no distinction between responses of this kind and those that are obviously directed towards the goal, but in practice it is only the latter that are recorded. As reaction potential increases, the proportion of preparatory responses that get "nipped in the bud" is likely to get much smaller, so that few of them will pass unnoticed.

To test our hypothesis we have data from the experiment described in the latency article. In this experiment 118 rats were taught to jump onto the entrance platform of a rectangular maze with a food box at each side. Both food boxes were open, and each contained 2 grams of food. The drive was 23 hours of hunger. For each rat preliminary training was continued until it reached a criterion of two successive unprompted jumps over a 6-inch gap, and on the following day the gap was widened to 8 inches. There were no other changes. The animals were classified into four groups on the basis of the number of days of preliminary training they required, and subsequent training was continued until all the animals in a group had reduced their jumping latencies to .7 seconds or less. Later the animals were reclassified on the basis of the number of days they required to reduce their latencies to .7 seconds or less, and it is this revised classification that we shall use here.

In addition to the reaction latencies, the experimenter recorded, by means of a stop-watch with a separate device for returning the hands to zero, the total time (t_p) during which the animal appeared to be reacting to the maze by preparatory movements, and also the number (n') of distinct preparatory movements. For this purpose actions of the following types were considered to be preparatory movements: (1) a move across the front of the platform from left to right or from right to left; (2) a movement of the head or of the head, shoulders and front legs, towards the right or left half of the maze entrance without bodily movement across the platform; and (3) a direct walk from the back of the platform towards the front.

It will be seen that some of these actions are very similar to typical VTE movements, although it is obvious that they cannot be explained by Tolman's theory, since no discrimination is called for. There is no difference in difficulty between jumps to the right, left and center of the entrance platform; and after the jump the animal is still free to choose between the

right and left alleys, so that even if discrimination is called for it comes into operation only after the animal has jumped. The movements we have recorded are therefore preparatory to the jump and not necessarily to the choice between alleys, and for that reason a simple movement from back to front of the platform, which would not be regarded as VTE, is legitimately included as a preparatory movement.

Table 2 presents the mean, standard deviation and coefficient of variability of ${}_sV_\rho$ at each trial for each of our four experimental groups. It also presents, in the 4th column of each section, $\sqrt{V_\rho}$, which is defined as the root of the ratio of the mean ${}_sV_\rho$ to the maximum. As in the case of ${}_sV_R$, the maximum velocity is taken to be 2, so that $V_\rho = \frac{{}_sV_\rho}{2}$. This has the effect that the beginning of the final ρ which merges into R is taken to be contemporaneous with the beginning of R. At the early trials, when the latency of ? is substantial, this identification of the beginning of the final ρ with the occurrence of R has little effect on the estimate of ${}_sV_\rho$; but at the later trials, when there is only one preparatory movement, and that passes without a break into R, it is obvious that the latency of ρ is overestimated. We had at first thought of meeting this difficulty by adding 2 instead of 1 to the number of recorded preparatory movements, but this proved to be excessive in that it gave rise to certain absurdities that came to light when the theory was applied to selective learning, and accordingly we decided to add 1 only.

This means that the maximum ${}_sV_\rho$ is taken to be too small, with the result that the early values of $\sqrt{V_\rho}$ are overestimated. (On the other hand, the addition of 2 to n', which implies that the maximum velocity is 4, probably underestimates $\sqrt{V_\rho}$.) At the later trials the number of animals for which $n' = 0$ increases, and since for each of these animals ${}_sV_\rho$ is underestimated, the mean becomes progressively dis-

TABLE 2

CONSTANTS OF THE ${}_sV_\rho$ DISTRIBUTIONS

Day	Group A (N = 33)				Group B (N = 28)				Group C (N = 39)				Group D (N = 17)			
	A	σ	C	$\sqrt{\frac{A}{2}}$	A	σ	C	$\sqrt{\frac{A}{2}}$	A	σ	C	$\sqrt{\frac{A}{2}}$	A	σ	C	$\sqrt{\frac{A}{2}}$
1	.220	.131	59.5	.332	.173	.094	54	.294	.125	.053	42	.250	.093	.034	36.6	.215
2	.331	.136	41.1	.407	.249	.104	42	.353	.166	.054	32.5	.287	.166	.059	35	.288
3	.662	.274	41	.575	.411	.169	41	.453	.212	.061	29	.326	.198	.062	31.3	.315
4	.838	.332	39.9	.647	.536	.192	35.8	.517	.378	.107	28	.435	.236	.066	28	.344
5	1.397	.438	31	.836	.796	.198	25	.631	.753	.207	27.5	.614	.267	.071	26.6	.365
6	1.822	.119	7	.955	1.040	.218	21	.722	.930	.238	25.6	.682	.317	.082	25.8	.398
7					1.643	.232	14	.908	.940	.231	24.6	.685	.465	.111	23.8	.482
8					1.779	.111	6	.944	.853	.301	35	.653	.588	.131	22.2	.542
9									1.310	.437	33	.810	.709	.151	21.3	.595
10									1.408	.391	28	.849	.944	.160	19	.650
11									1.458	.341	23	.855	1.031	.223	21	.718
12									1.700	.153	9	.923	.844	.244	29	.650
13													1.021	.235	23	.714
14													1.194	.300	25	.770
15													1.676	.226	13	.915
16													1.735	.136	8	.931

TABLE 3

CONSTANTS OF THE ${}_sV_R$ DISTRIBUTIONS

Day	Group A (N = 33)				Group B (N = 28)				Group C (N = 39)				Group D (N = 17)			
	A	σ	C	$\sqrt{V_p}$	A	σ	C	$\sqrt{V_p}$	A	σ	C	$\sqrt{V_p}$	A	σ	C	$\sqrt{V_p}$
1	.031	.026	83	.124	.023	.022	97	.107	.016	.018	112	.090	.008	.007	88	.062
2	.064	.064	100	.179	.054	.049	90	.164	.030	.026	82	.123	.017	.017	100	.092
3	.275	.199	72	.371	.127	.112	88	.252	.060	.046	77	.173	.031	.029	94	.125
4	.629	.324	51	.560	.329	.201	61	.405	.129	.083	65	.254	.048	.038	79	.155
5	1.397	.438	31	.836	.725	.226	31	.602	.216	.116	54	.328	.085	.046	54	.206
6	1.822	.119	7	.955	1.040	.218	21	.722	.381	.231	61	.436	.175	.117	67	.296
7					1.643	.232	14	.908	.593	.279	47	.544	.303	.233	77	.390
8					1.779	.111	6	.944	.853	.301	35	.653	.403	.255	63	.450
9									1.310	.437	33	.810	.491	.272	55	.495
10									1.408	.391	28	.849	.579	.300	52	.538
11									1.458	.341	23	.855	.679	.263	39	.583
12									1.700	.153	9	.923	.844	.244	29	.650
13													1.021	.235	23	.714
14													1.194	.300	25	.770
15													1.676	.226	13	.915
16													1.735	.136	8	.931

torted. This is clearly seen in group C, where the mean drops from .940 at the 7th trial to .853 at the 8th trial. The mean $n' + 1$ at the 7th trial was 1.3, and at the next trial it fell to 1.0. That is to say, about 30 per cent of the animals for which the estimated ${}_sV_p$ was reasonably correct at the 7th trial were added at the 8th trial to the group for which ${}_sV_p$ was underestimated. This sudden increase in the proportion of underestimated values is reflected in the means. There is a similar phenomenon in group D. In groups A and B there is no inversion of the ${}_sV_p$ curve, but there appears to be an inflection in group B at the point where n' becomes zero.

Since our deductions involve comparisons between ${}_sV_R$ and ${}_sV_p$, the relevant data from the reaction latency article are reproduced in Table 3.

A comparison of the A columns in Tables 2 and 3 shows that our first deduction is confirmed, but this in itself, as we have already stated, is of no scientific significance.

Before considering the remaining deductions it is necessary to refer to a discrepancy between theory and observation which escaped notice in the reaction latency article. In that article it was deduced that the standard deviation of ${}_sV_R$ would rise to a maximum when the distribution was symmetrical and then decline. Examination of Table 3 shows that the standard deviation reaches its maximum quite near the end of learning, whereas symmetry of the distributions is reached about the middle. The standard deviation continues to increase while the form of the distribution is changing in the direction of negative skewing.

A possible explanation of this discrepancy is suggested by Hull's mimeographed Memorandum of May 16, 1950, in which it is deduced that "The distribution of reaction latencies, conditioned, *e.g.*, to the subsident phase of the stimulus trace, will have its median in the recruitment phase, skewing off with a relatively long tail in the subsident phase of the trace." This implies that there is a source of

variability in reaction latency, and hence in velocity, over and above that discussed in the latency article. There it was assumed, although not explicitly stated, that the stimulus acts continuously and uniformly until its effect is terminated by the response. The strength of the stimulus-trace will of course vary a great deal while the animal is on the jumping platform, especially in the early stages of learning, when it frequently turns its back on the stimulus. But each preparatory movement involves a new presentation of the stimulus, so that the trace is continuously being renewed and held at a relatively high average strength. As latency diminishes, and with it the number of preparatory movements, it becomes increasingly probable that the animal's behavior is determined by a single snap-shot presentation of the stimulus. In that case the stimulus-trace can no longer be regarded as constant, but must be assumed to increase rapidly and diminish slowly, so that its value at the moment of response-evocation becomes an additional source of variability in the velocity both of the response itself and of the preparatory movements.

At the beginning of learning, when ${}_sE_R$ is low, the reaction is likely to be evoked only when the stimulus-trace is at or near its maximum value. With increase in ${}_sE_R$ progressively lower values of the trace become adequate to evoke the response, so that variability arising from the course of the stimulus-trace becomes an increasingly important factor. Hence, towards the end of learning this additional source of variability is likely to delay the diminution of the standard deviation predicted from the binomial theorem.

Since we have assumed that the threshold for preparatory movements, ${}_sL_p$, is lower than ${}_sL_R$, it follows that low values of the stimulus-trace may be effective in evoking preparatory movements at a time when high values are still required for the evocation of a jump. Hence the additional source of variability due to the course of the stimulus-trace becomes effective in relation to preparatory movements before it begins to affect the complete response.

In order to see how variability arising from the stimulus-trace affects the problem, we may consider a skew frequency distribution of latencies such as Hull has deduced. Figure 2A represents such a curve plotted against a scale of latencies. The scale values have been chosen in such a way as to yield a range of velocities such as we find near the end of learning. In Fig.

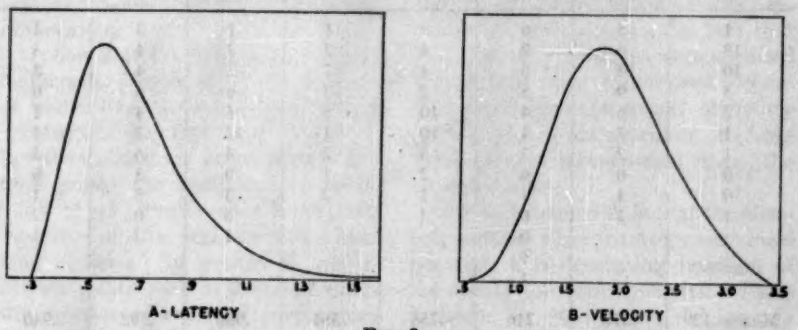

FIG. 2.

2B the scale of latencies has been converted into a linear scale of velocities and reversed in direction. The ordinates of curve A have been plotted against the corresponding velocities to produce curve B. It will be seen that the velocity curve is slightly skewed in the positive direction, and this effect would be more marked if the minimum latency were taken to be .2 instead of .3. Latencies of .2 are in fact not infrequent in the case of preparatory movements, and if all such movements could be detected, the minimum latency might prove to be even smaller, with the result that the distribution of velocities would be more heavily skewed in the positive direction.

The two sources of variability, deduced from the stimulus-trace and from the binomial theorem, thus result in distributions that are skewed in opposite directions, and the empirical distributions of $_sV_\rho$ will therefore be more nearly symmetrical than the binomial theorem would lead us to expect.

This deduction is confirmed by our empirical data. As a typical example we present in Table 4 the distributions of $_sV_R$ and $_sV_\rho$ for the 4th to the 7th days of group C. The scales are different in each case, but these are not presented, since we are here concerned only with the shape of the distributions. On the 4th day the distribution of $_sV_R$ is positively skewed, but that of $_sV_\rho$ is more nearly symmetrical, in accordance with the deduction from the binomial theorem. On the following days the distribution of $_sV_R$ becomes more symmetrical, but there is only a slight tendency to change in the direction of negative skewing in the $_sV_\rho$ distributions. Inspection of the mean velocities, in the last row of the table, shows that the latencies of R are greater throughout than those of ρ. On the 5th day, for example, the latency corresponding to the mean $_sV_R$ is 4.6 sec., while that of $_sV_\rho$ is 1.3. Variation in the stimulus-trace is therefore more likely to be effective in modifying the dispersion of $_sV_\rho$ than of $_sV_R$. That is to say, the change in the direction of negative skewing is likely to be masked.

Tables 2 and 3 show that the standard deviation of $_sV_\rho$ apparently reaches its maximum at about the same time

TABLE 4

4th day		5th day		6th day		7th day	
$_sV_R$	$_sV_\rho$	$_sV_R$	$_sV_\rho$	$_sV_R$	$_sV_\rho$	$_sV_R$	$_sV_\rho$
1	5	6	2	11	1	3	2
18	6	9	4	7	3	4	3
10	4	6	4	7	2	4	8
5	9	5	2	3	6	6	6
2	6	4	10	5	4	4	1
1	5	4	10	1	12	2	13
1	2	3	3	2	4	6	2
0	0	0	2	1	0	3	0
0	1	1	1	2	3	5	1
1	1	0	1		3	0	2
		0			1	2	1
		0					
		1					
Means .129	.378	.216	.753	.381	.930	.593	.940

as that of ${}_sV_R$. This is not in accordance with the deduction from the binomial theorem, but there are two factors that probably distort the results. In the first place, when the number of recorded preparatory responses falls to zero, ${}_sV_p$ is assumed to be identical with ${}_sV_R$, and is therefore underestimated. It is not impossible that if the true values of ${}_sV_p$ could be determined, their dispersion would turn out to be more skewed than that of ${}_sV_R$ and the standard deviation would be lower.

The second distorting factor is the variability arising from the course of the stimulus-trace. Since we have assumed that this variability becomes increasingly important as the superthreshold value of ${}_sE_R$ increases, it follows that at any stage this factor plays a relatively greater part in determining the dispersion of ${}_sV_p$ than of ${}_sV_R$. Hence at a time when the dispersion of ${}_sV_R$ is symmetrical and is still mainly determined by the binomial expansion, the variability of ${}_sV_p$ is being largely influenced by the stimulus-trace. As this factor tends to produce a tail in the positive direction, while the binomial distribution has a tail in the negative direction, the standard deviation must be greater than the binomial theory predicts. We submit therefore that there is no essential conflict between theory and observation.

Tables 2 and 3 show that our next deduction, concerning the coefficient of variability, is confirmed, although here again the distorting factors referred to above are in evidence. For each group the coefficient of variability of ${}_sV_p$ begins at a lower level than that of ${}_sV_R$, and diminishes from trial to trial. In groups C and D there is an inversion at the point where the number of recorded preparatory responses vanishes, but this is fully explained by the errors due to confounding ${}_sV_p$ with ${}_sV_R$.

TABLE 5

Trial	Group A	Group B	Group C	Group D
1	4.06	5.04	8.20	10.91
2	2.15	2.45	3.24	6.74
3	1.30	1.53	2.83	4.28
4	1.03	1.14	2.09	3.75
5	1.00	1.03	2.00	3.43
6	1.00	1.00	1.64	3.06
7		1.00	1.32	2.91
8		1.00	1.00	2.33
9			1.00	2.11
10			1.00	1.56
11			1.00	1.34
12			1.00	1.00
13				1.00
14				1.00
15				1.00
16				1.00

The 4th column for each group in Tables 2 and 3 shows that our deduction concerning the proportion of the ${}_sO_R$ curve above the threshold is confirmed. The proportion above ${}_sL_p$ is in each case greater than the proportion above ${}_sL_R$ at the beginning of learning, and rises to the same level at the end. The curve is thus a truncated ogive, as we have deduced. It is of some interest to note that the initial level of this curve is a decreasing function of the number of reinforcements required to reach the criterion of learning. This may be readily explained by reference to the fact that two unprompted jumps were required before the main experiment began. The rapid learners would obviously acquire a larger increment of ${}_sE_R$ from these two reinforcements than the slower learners.

Our final deduction is that the absolute number of preparatory responses per trial is a decreasing function of the number of reinforcements. Table 5 shows, for each group, the average number of such responses at each trial,

including the movement which merges into the jump.

The number of preparatory movements shown here is very much smaller than the theoretical numbers shown in Table 1, but this is to be expected in view of the difficulty of detecting any but overt responses. In spite of that, the number of movements decreases from trial to trial, as we predicted.

It will be observed that the number of preparatory movements in the first trial increases from group A to group D. This too is in agreement with theory. Although our deduction was stated as a relationship between the number of movements and the number of reinforcements, it is obvious that this relationship depends on (a) the relation between ${}_sE_R$ and the number of reinforcements, and (b) the relation between the proportions of the ${}_sO_R$ curve above the two thresholds and ${}_sE_R$. The number of preparatory movements is directly determined by the latter relationship. As we noted above, the level of ${}_sE_R$ at the first trial is highest for group A and lowest for group D. Hence the increasing number of movements from A to D represents a further empirical confirmation of our hypothesis.

The present discussion deals with behavior in a situation which involves no choice between alternative responses, and therefore gives no opportunity for VTE as defined by Muenzinger. We believe that VTE is essentially a succession of preparatory movements directed towards alternative goals, and in that case it should be amenable to treatment by methods analogous to those we have applied to such movements in a situation where there is only one goal. A paper on this subject is in course of preparation.

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WHAT IS A FORM?[1]

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The term *form* is used by different people to mean different things and by the same person to mean different things on different occasions. It can refer to the curved surfaces of a human female or the contours of a crankshaft, to a polyhedron or the style of a man's tennis game. *Shape, figure, structure, pattern, order, arrangement, configuration, plan, outline, contour* are similar terms without distinct meanings. This indefinite terminology is a source of confusion and obscurity for philosophers, artists, critics, and writers. It is an even more serious difficulty for scientists and psychologists. Ambiguity is excusable in the preliminary exploration and discussion of a problem, but it cannot be tolerated when a theory has reached the stage of experimental verification. A more rigorous terminology is very much needed. The psychological problem of how animals and men *perceive* form requires a definition of what it is that is perceived. Experiments in the field of form-perception and constancy of shape can only be decisive if one experimenter knows what the other is talking about.

When the environment of an individual is said to consist of objects, places, and events, a rough threefold classification of "formal" properties is suggested. One ordinarily applies the term *form* to an object, *arrangement* to a place, and *order* to an event. We might agree that the perception of a single object (one delimited by a surface), the perception of a set of objects (a region of space), and the perception of what happens to objects (movement and sequences in time) are three distinguishable problems each of which deserves a terminology of its own. The terminology with which this paper is concerned is the first. The only kinds of visual form we shall undertake to deal with here are those associated with or derived from physical objects. Perhaps the simplest kind of shape is that embodied in an isolated object. If so, the effort at definition should begin with this kind.[1]

Even in this limited sense there seem to be at least three general meanings for the term form. There is first of all the substantial shape of an object in three dimensions. Second, there is the projection of such an object on a flat surface, either by light from the object or by the human act of drawing or the operation of geometrical construction. Images, pictures, drawings, and outlines are examples of form in this second sense. Third, there is the abstract geometrical form composed of imaginary lines, planes, or families of them. In this last sense of the term, form is

[1] This inquiry is connected with a series of experiments on the visual perception of the environment performed under Contract AF41 (128)–42 between Cornell University and the USAF School of Aviation Medicine.

[1] There are, of course, many other formal properties of the environment even beyond those listed, such as the expression of a face, the composition of a painting, and the structure of a family. All these are unquestionably perceived, but they are so complex that the task of the psychologist is probably one of exploratory experiment rather than attempting to make exact definitions. Some psychologists in the tradition of phenomenology have tried to describe these higher-order varieties of form. The *overall* quality of a picture or a poem which makes it "good" would not be made any more intelligible, however, by imposing a terminology on critics of art and literature. The writer is only proposing that we be precise at a simple level where it may be profitable.

said to be farthest removed from "substance." The reservation should be noted, however, that geometrical forms are presented to students as perfectly substantial black marks on white paper.

If one simply puts the question, How do we perceive form? without distinguishing between these three meanings of the term, a clear answer can hardly be given. Nevertheless, the problem of form-perception in psychology has usually been put in this way. What are the main solutions to the problem of form-perception? One kind of answer is to suppose that man simply has a form-sense. This is in effect no answer at all. It is still accepted, however, especially among ophthalmologists and physiologists. An alternative kind of answer is to assert that man somehow *learns* to perceive form. This answer is also widely accepted among some psychologists, although no one has been able to show just how this learning might occur. A third and more sophisticated answer is that of the Gestalt theorists who propose the general formula that the excitations on the retina are converted into forms by a process of organization in the brain, and who look to the general "laws of form" for an explanation of form-perception.[2] All these answers seem to take it for granted that a form is simply a form and that since everyone knows what the term means there is no need to specify it. Whether the form referred to is a physical thing or is an abstract property of a physical thing is not clear. Experimenters sometimes refer to *a form*, which implies that it is concrete, and at other times to the *form of a thing*, which implies that it is abstract. The only way to discover what they mean is to examine what they put in front of the observer's eyes during the experiments. In the vast majority of studies of form-perception it is artificial deposits of one or another sort on a paper surface. The theories of visual form, on the whole, have been based on evidence obtained with outline drawings.

It will be argued here that drawings are particularly inappropriate objects with which to begin a study of the perception of form. A drawing is a human production never found in a natural environment; it is complicated by being a thing with which men communicate with one another; it is not a simple presentation to sense-organs but a *representation* or a substitute-object. As a stimulus for perception it is convenient, but it is far from being the primary or fundamental stimulus which psychologists have usually taken it to be.

The reasons for supposing that the primary kind of form is a drawn form would make a long chapter in the history of scientific thought. A main reason, however, is the classical assumption that two-dimensional vision is immediate, primitive or sensory, while three-dimensional vision is secondary, derived, or perceptual. One must first see a plane form before one can see a solid form. This notion is connected with the argument that the three-dimensional properties of things can have no correlates in a two-dimensional retinal image, and that the three-dimensional properties must therefore be reconstructed by the mind or the brain. The writer has suggested elsewhere that this argument is a fallacy (1). So far from plane vision being primary and solid vision secondary, it is the other way around:

[2] The original laws of visual organization were formulated by Wertheimer (8) on the basis of observations with spots, lines and outlines, and their implications were developed by Koffka (4). The effort of Köhler to explain form-perception (5, 6) appears to be a search for laws of electrical current flow in the brain rather than laws of perceived form. Köhler believes, however, that these will ultimately prove to be the same, on the basis of his hypothesis of "isomorphism" between cortical process and phenomenal percept.

There is overwhelming evidence to show that solid vision is primary and that plane vision is acquired only with training and by adopting a special attitude. The impression of a visual *world* may well prove to have a straightforward explanation; the impression of a visual *field*, however, is a very sophisticated kind of seeing and its explanation is far from being simple.

It is possible to revise the traditional view that our visual sensations are two-dimensional and our perceptions are three-dimensional. When the doctrine is thus turned upside down it may appear strange on first inspection, but its intelligibility is much improved. The existing theories of form-perception aim first at two-dimensional form. If they fail to be convincing, the reason may be that three-dimensional impressions which have the property of shape are actually easier to account for than two-dimensional impressions which have the property of shape. Perhaps the theories have been off on a false scent.

After a criticism as sweeping as the above, it is only fair to invite some return. A set of definitions and distinctions will therefore be proposed for the main types of visual form. If they are unacceptable, at least they will be clearly so. What are the various intelligible meanings that can be assigned to the term?

1a. *Solid form.* The closed physical surface enveloping a substance of some kind; the margin between two states of matter (usually between a solid and air). The surface may be curved or it may be composed of adjoining flat surfaces with edges; the former type can always be treated mathematically as a special case of the latter when the number of flat segments is very large. Organisms tend to have curved surfaces and to change their form with growth (7); fabricated objects and a few natural objects like crystals tend to have adjoining flat faces and to resist transformation.

Objects have solid forms in the sense defined. When we perceive a detached object we also *see* a solid form—the depth, relief, or modelling of the surface. How we do so is a problem of long standing.

1b. *Surface form.* A flat physical surface with its edges; the face of an object (or one of the faces of a thin sheet of material such as paper). A surface-form always has an orientation which we shall term *slant*. Slant can be defined as the angle of inclination of the surface to the line of sight or, if preferred, to the axis of gravity (2).

Perceiving a surface-form involves perceiving both the slant of the surface and the form of its edges; an impression of form is never obtained without some accompanying impression of the angle at which the surface lies, either frontal or inclined. The problem of shape-constancy, so-called, is better formulated as the problem of seeing shape-at-a-slant.

If the modelling of a solid form is reducible to the varying slants of its faces, a solution for the problem of how we see slant ought to provide a solution for the problem of depth and relief. The slant of a surface is a physical variable which is simpler to define and easier to manipulate than is the modelling of an object.

2a. *Outline-form.* Physical tracings made with ink, pencil, or paint on a surface, which geometrically *represent* the edges of a surface-form or the margins of a solid form. These tracings have a finite thickness; they are drawn lines rather than the theoretical lines of geometry. They have two margins instead of the one margin exhibited by the edge of an object.

The perception normally aroused by an outline-form is quite unlike the outline itself. The paper surface is scarcely seen and a different surface seems to

emerge within the outline. The paper surface appears to become "background" and to recede while the inclosed surface seems to take on "figural" qualities and to stand out (4). This, however, is a sophisticated report. Most observers perceive an object and do not see tracings on a surface at all. When you press the question, however, they tell you that they do not literally see a physical object but a picture of it. Hence the perception is not like that of a solid form.[3]

2b. *Pictorial form.* Any representation of a physical object on a surface by drawing, rendering, painting, photography or other means. This would include outlines, silhouettes, plan-views, engineering drawings, and perspective drawings; it extends to transparencies, images projected on a screen, motion pictures and in short, to the vast variety of things we call pictures. One feature is generally to be found—a frame, usually rectangular, edging the surface on which the representation appears. A pictorial form is normally presented to the eyes with the surface perpendicular to the line of sight, that is, at a zero slant.

Some pictures represent other physical properties of an object in addition to the margins and surfaces of the object, such as color, texture, shading, and motion. A color photograph or a 16th century Dutch still-life are examples. If a sufficient number of variables has been incorporated in the deposits of pigment or dye on the surface, an instructive result may be achieved by an ingenious experimenter. He may fool an observer into believing he sees a real object instead of a picture. When it is carefully arranged that the picture is seen through an aperture so that the frame is invisible, the head is motionless, and only one eye is used, *the resulting perception may lose its representational character.* This may be termed a "peephole situation." In these circumstances, a pictorial form is equivalent to a solid form or, as we say, the observer has the illusion of reality. It may be noted that his retinal image closely resembles the one he would have in an actual peephole situation with a solid form.[4] What this demonstration brings out is the fact, often forgotten, that a pictorial form *as ordinarily viewed* induces a quite different type of visual perception from that of a solid form.

Any pictorial form, including the special case of a simple outline form, has been defined as a representation of an object. We therefore need a definition of a representation. How is one made? The fundamental types are (a) the plan and (b) the perspective of a surface-form.

2c. *Plan-form.* Outlines indicating the plan-projection or "plan-view" of the edges of a surface form. A plan-view is exemplified by an engineering drawing. It does not involve a transformation. The terms projection and transformation will be defined later.

2d. *Perspective-form.* Outlines indicating a perspective-projection or "perspective view" of the edges of a surface form. This always involves a transformation (relative compression or foreshortening). We say that such a drawing shows the object *in perspective.*

[3] A corollary of this definition is that the figure-ground phenomenon has been derived fro 1 the perception of outline-forms, not from a study of all forms or of all perception. The universality of the phenomenon as ordinarily described is therefore questionable. It is one of the most convincing tenets of Gestalt theory in its battle with elementarism but whether it will serve as the fundamental basis for a complete theory of perception is not so certain.

[4] Other techniques of pictorial viewing which aim at the complete illusion of reality, notably stereoscopic movies, are impressive but fail to achieve this end. As long as the frame is visible, a picture will look like a picture.

It is true that, after training, adults can visualize the perspective-form of an object they see without having a drawing or picture in front of them. This training is what enables artists to *make* perspective drawings without using special optical or geometrical techniques. Children, in general, cannot do so. This ability to visualize a thing on a picture-plane is probably what lends plausibility to the unfortunate doctrine that we have sensations of form mediated by a "form-sense." The assumption is that the retinal image of a three-dimensional object is a perspective picture in two dimensions and that hence the resulting sensation must be a perspective-form in two dimensions. The doctrine is then faced with the knotty problem of how the sensation can be converted into a three-dimensional perception.

2e. *Nonsense-form.* Tracings on a surface (a pictorial form) which do not specifically represent (are not a projection of) a recognizable object. A drawing may be meaningless because (a) the projection is crude or inaccurate or (b) the tracings are accidental, like a child's scribble or the contours of an ink-blot, or (c) the tracings have a plan or system not *designed* to be a projection of a recognizable object. These latter are what modern artists often construct and call *abstract forms*, but they should be distinguished from the abstract geometrical forms to be defined later. They are also called non-objective forms or non-representational forms, and these terms are better. It should be noted that a drawing may also be meaningless for a quite different reason, because, although it is an accurate projection of an object, the object is not recognizable to the observer. Biological drawings and mathematical constructions are often of such a nature.

The fact is that nonsense-forms are never nonsensical; they are never actually meaningless to an observer, but are simply unspecific or ambiguous. The perceiver discovers a succession of objects in the picture or, if not objects, then surfaces, edges, and fanciful constructions which are often aesthetically interesting.

We come next to the genuinely abstract forms of geometry. They are certainly not substantial surfaces and edges although they are just as certainly connected with these things. They are also not tracings on paper, although they are represented or symbolized by such. They are in a class by themselves.

3a. *Plane geometrical form.* An imaginary closed line on an imaginary plane. A geometrical line has no width and a geometrical plane has no thickness. A geometrical line is indicated or suggested by a substantial tracing on a substantial surface, but the two should not be confused. Geometrical lines and planes can be specified by the equations of analytic geometry more accurately than they can be drawn on paper. For the practised mathematician equations are often preferable to drawings. Geometrical forms are infinitely variable and only a very few special cases of them have names. Words like *triangle, rectangle, square, circle* stand for only the most familiar geometrical forms.

3b. *"Solid" geometrical form.* An imaginary closed surface in an imaginary space of three dimensions. The forms of solid geometry are, of course, no more solid than the blue sky. They are the prototype of all ghosts. They are, in fact, the ghosts of objects just as planes are the ghosts of surfaces, lines the ghosts of edges, and points the ghosts of particles. We can conceive a geometrical form but we cannot see it in the same sense that we see an object, for the form is an abstract property of many objects.

3c. *Projection. Projected form.* A

geometrical form on one plane which is in an exact correspondence with a form on another plane, the correspondence being defined as point-to-point or one-to-one. The relation between a form and its projection is physically exemplified by an object and its shadow (silhouette) or an object and its pinhole-image, that is to say by optics. If the planes of the two forms are parallel, the forms are geometrically similar or congruent, like scale-drawings or plan-views; if the planes are not parallel, one form is a perspective-transformation of the other.

Conceived thus, a given plane geometrical form is only one of an infinite set of perspective-transformations. The physically analogous fact is that when an objective surface-form is projected on another surface by light, the differing orientation of the form to the surface yields a set of different perspective-forms. The *psychologically* analogous fact, one might suppose, would be that when a surface-form is viewed at different angles of regard, the perceiver obtains a set of different perspective-impressions. The difficulty for psychology is that under ordinary circumstances he does not; instead he obtains a constant percept of the surface-form with a varying impression of slant.

The Problem of Form-Perception

The above definitions provide a terminology which can now be applied. Returning to the original question of how we perceive form, the obvious reply must be, what *kind* of form? The question must be divided into three or more questions. Solid or surface forms, pictorial forms, and geometrical forms —these at least must be treated separately.

Perceiving surface-forms. If the psychologist can explain how we see a given face of a solid object having a certain form at a certain slant, the explanation of how we see the whole object in three dimensions can be derived. This is the problem of shape-constancy. It has usually been assumed, in thinking about shape-constancy, that seeing a form without slant was simpler than seeing a form with slant. Since the retinal image is flat, there is supposed to be a retinal form, a two-dimensional form, or a "pattern-stimulus," which initiates the process of perception. But what could this retinal form possibly be? It is clearly neither a substantial form nor an abstract geometrical form. Perhaps it is a pictorial form—the "retinal picture" of commonsense psychology. Nothing, however, could be more mistaken. A picture is something to be looked at. The retinal image could only be a picture if there existed a perceiver behind the eye to look at it. The retinal image is none of the kinds of form defined; ***it is in fact not a form at all.*** It is a complex of variables of light-energy, definable in terms of steps and gradients but not in terms of physical edges, geometrical lines, or graphic outline (1). Ordinarily there is a dual complex of energy (a pair of images) on a bifurcated receptor-surface (a pair of retinas). We do not see our retinal images; we see an object, and the process is mediated by the images. The images as such may prove to be definable in terms of *order* (perhaps the kind of order exemplified by the number-series) but not *form*. The writer believes that the retinal images should be conceived as a kind of "ordinal stimulation" (1, Chap. 5).[5]

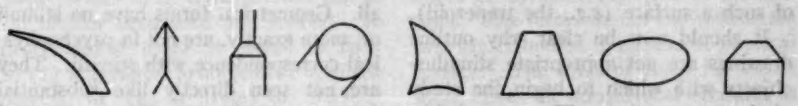
In order to understand the perceiving

[5] In connection with the analysis of visual images in terms of variables, it is interesting to note that closed geometrical forms are variables when one considers their perspective transformations, and that different kinds of geometrical forms (*e.g.*, triangle, square, circle) can probably be specified in terms of variables, although little mathematical effort has been expended in this direction.

of a surface-form, therefore, the problem is to specify the variables of stimulation which elicit the perceived properties of the object—the slant of the surface together with the form of its edges. Both the surface and its edges are specifically given in retinal stimulation but they are not represented there. The pair of images is a correlate but not a copy of the object. The nearly constant appearance of these edges at differing slants in the case of ordinary naive perception will probably prove to be simply a by-product of the specifying of this stimulation. The dependence of phenomenal slant-shape upon the impression of slant may then be clarified.

Perceiving objects mediated by pictures. The question of what and how we see when we look at a picture of some kind is quite different from the question formulated above. The process of object-perception is surely simpler than the process of picture-perception. Despite the fact that men have been making drawings for thousands of years and during the past century have invented a variety of displays which our ancestors never dreamed of, we know very little about what happens within us when we see a picture. This much is certain, however. Pictures are *on* a surface whereas substantial objects *are* a surface. Moreover pictures *stand for* substantial objects in addition to *being* substantial objects. An outline-form, a painting, a photograph, or a radar screen-picture are each a sign of something else. What the observer ordinarily perceives is the object, place, or event represented, and this fact poses a special problem of perception.

Take for example an outline-form, *i.e.*, a line drawing on paper. It is almost impossible to avoid seeing the properties of a surface-form having edges which stand out from the background. This "piece of surface" may have a form and a slant quite different from the form of the tracings and the slant of the paper. This last fact is so important that an experimental demonstration of it is worth reporting.



The outline-forms illustrated, drawn on cards, were shown in succession to an observer with the instructions, "Tell me what you see *on the card*. Keep looking at it, and if what you see changes describe it also." For each card the verbal descriptions were recorded and later classified. Every *O* reported two or more perceptions within 60 seconds and some had as many as seven. Ten *O*s were used.

At no time did any *O* describe anything like black deposits or marks or traces on a white surface. All the terms and phrases used fell into three other classes: *lines and angles, geometrical figures,* and *solid objects with physical surfaces.* The first two kinds were very infrequent; the great majority of terms referred to objects. Evidently what every *O* saw "on the card" was seen with what might be called the pictorial attitude. The physical objects reported were highly variable, differing from one *O* to another and successively for the same *O*. They could be divided into two sub-classes: (1) objects for which the outline-form was a plan-view, and (2) objects for which it was a perspective-view. With only one or two exceptions, *every observer saw every*

outline-form in each of these two ways. The first drawing could be a horn or a road curving up a hill, the second an arrow or a tent-roof, the fourth a pulley with rope or a cannon-muzzle, the sixth a truncated pyramid or a carpet on the floor, and so on. It is obvious that a single outline-form may elicit perceptions of two quite different solid forms. The form of a surface with its edges (*e.g.*, the carpet) may be very different from the outlines representing the edges of such a surface (*e.g.*, the trapezoid).

It should now be clear why outline drawings are not appropriate stimulus-objects with which to begin the study of form-perception. They are habitually taken to stand for something other than what they are and, more important, what they stand for is often equivocal. An outline, representing as it does only the edges of a surface, may stand for any *object which projects that particular outline,* including some very queerly shaped surfaces. For instance, a given trapezoid, or a trapezoidal pencil of light-rays, may stand for a square at a given slant but it may also stand for any of an infinite set of different trapezoids at other different slants. Conceiving the matter in this way, it is only to be expected that the perceiving of outline-forms is fluid, changeable, and seems to have a spontaneous character. The perceptions they arouse are unstable because they are equivocal representations. It is not necessary to infer that the stimulus-distribution is unstable, nor to suppose that it moves in the direction of equilibrium or "good form." Nearly all the research on form-perception has utilized outline-drawings as stimuli. If it be granted that these are actually *pictures of forms,* the research is irrelevant to the problem. A genuine psychophysics of form-perception will have to deal with "shape-slant," *i.e.*, with transformations of form which co-vary with degrees of slant. In contrast with an outline drawing, a pair of retinal images of the usual sort contains stimulus-variables for the perception of slant (2). Only in "peephole situations" are these stimulus-variables so impoverished that the perception of slant becomes ambiguous.

Visualizing geometrical forms. To the question of how we perceive form in the third general meaning of the term, the answer is probably that geometrical forms are not perceived at all. Geometrical forms have no stimuli or, more exactly, are not in psychophysical correspondence with stimuli. They are not seen directly like substantial forms, or indirectly like represented substantial forms, but instead are conceived or abstracted from innumerable past seeings of both. Not much is known about the process of abstraction or concept-formation, but it is fairly certain that a child can identify a simple object at a very early age, a represented object at a later age, and a concept at only a much later age. The geometrical forms we are talking about are conceptual or general. It must be remembered that the geometrical triangle referred to in a theorem is a triangle in general, not just the particular form in the geometry textbook.

What kind of forms *are* the drawings in the textbooks then? One might be tempted to say they are pictorial forms, but this would not be strictly correct. Outline-forms do not represent geometrical forms in the way that they represent the edges of surface-forms, or objects. A geometrical drawing may be said to signify a whole set of projective transformations. A set of transformations is even more ghostly than a single geometrical form. An outline taken in this sense is more nearly a symbol than it is a picture. The relationship of *standing for* is more dependent on an arbitrary convention, and therefore on learning, than it is in the case of a pic-

ture. Consequently an outline-form presented to an observer without any other indications of the object represented is even more ambiguous than it was made out to be in the last section. In addition to being equivocal as a picture, it is a symbol for a bevy of geometrical ghosts. To assume that it constitutes a simple "stimulus" for perception is completely misleading.

Patterns and textures as distinguished from forms. The term pattern has so far neither been used nor defined. In psychological usage it seems to refer to (1) a group or arrangement of single objects, or (2) a group of artificial traces on a surface, such as the patterns used to exemplify Wertheimer's laws of visual organization, or (3) a group of natural inhomogeneities on a surface. In the latter meaning, a pattern passes over into being a *texture*. The latter term, in the writer's opinion, should mean an arrangement of visible particles not *on* but *in* a surface—the visible structure of a surface itself. This is important because of the possibility that the optically corresponding texture of the retinal image of the surface is the adequate stimulus-condition (or one of the stimulus-conditions) for the impression of the surface (2). The writer has suggested that the impression of a surface is essential for the perception of determinate visual space (1). The interrelationships between visual acuity, visual texture, surface-perception, and space-perception remain to be defined.

The first and second meanings of the term pattern—a grouping or arrangement—need definition and analysis as much as any of the others, but the task is beyond the scope of this paper. So also do "structure," "sequence" and the higher order varieties of *Gestalten*. The feeling of mystery that attaches to all such words ought to be dispelled, because they will be even more interesting when comprehended.

Conclusions. A number of explicit definitions of visual forms have been proposed out of a conviction that psychologists should come down to earth and say exactly what they mean when they talk about form. The suggestion was made that the kind of forms heretofore studied—pen or pencil tracings—is artificial, and as a type of stimulus for perception is equivocal. Such forms are either projections of disembodied edges or symbols for ghostly abstractions. The forms we need to investigate first are embodied in chunks of physical surface. The kind of form for which there exists an unequivocal stimulus is a form imbedded in a surface—that is to say, a shape-at-a-slant. When a form is not imbedded in a surface (and when, as a result, contour-stimulation is not accompanied by surface-stimulation) the resulting percept is ambiguous. Since the slant of the presumed surface is equivocal, the form is also equivocal, and what the observer sees is open to the influence of assumed probabilities, clues, unconscious inferences, the standards of past experience, or the social norms of group life. Since nearly all the experimental research on form-perception has been performed with outline-stimulation alone, we are tempted to conclude that *all* form-perception depends on probabilities, inferences, and norms—in other words, on subjective factors. This conclusion is unwarranted. Important as these factors no doubt are, the primary problem for psychologists is to isolate the invariant properties in visual stimulation which are in psychophysical correspondence with constant phenomenal objects. According to the proposed definitions *solid* forms and *surface*-forms are realities. *Outline*-forms and also *pictorial-*, *plan-*, *perspective-* and *nonsense*-forms are representations which the perceiver takes to stand for realities. For these a special theory of picture-perception is required. *Geo-*

metrical forms, both *plane* and *solid*, are abstractions which cannot even be represented, strictly speaking, but can only be specified by symbols. If the definitions are accepted, there is no such thing as form-in-general with the universal characteristics ascribed to it by Gestalt theorists. None of the above forms is a whole which is different from its parts. None is organized in any special sense. None is in the least dynamic. It is possible to understand, however, why these characteristics have been ascribed to visual *Gestalten*. The reason is probably that we have studied only the disembodied varieties of form—*i.e.*, ghost shapes—which are ambiguous representations or equivocal symbols, and which consequently yield fluid, variable, or inconsistent percepts.

If such be the case, the effort to determine what happens in the brain when one perceives form-in-general will prove to be fruitless. Theories such as those of Köhler (5, 6) and, more recently, Hebb (3) seem to be efforts of this sort. At least three separate levels of theory will be required: first, a theory of how we perceive the surfaces of objects—a theory of slant-shape or, in older words, of shape-constancy; second, a theory of how we perceive representations, pictures, displays, and diagrams; and third, a theory of how we apprehend symbols. There is no reason to suppose that the physiological concomitants of all these experiences will be the same; in fact, since pictures and symbols presuppose objects, their physiological explanations will probably have to be found at increasing levels of complexity. When these three levels of theory have been developed, the category of "form-perception" in psychology will have evaporated.

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A MODEL FOR STIMULUS GENERALIZATION AND DISCRIMINATION

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INTRODUCTION

The processes of stimulus generalization and discrimination seem as fundamental to behavior theory as the simple mechanisms of reinforcement and extinction are to learning theory. Whether or not this distinction between learning and behavior is a useful one, there can be little doubt that few if any applications of behavior theory to practical problems can be made without a clear exposition of the phenomena of generalization and discrimination. It is our impression that few crucial experiments in this area have been reported compared with the number of important experiments on simple conditioning and extinction. Perhaps part of the reason for this is that there are too few theoretical formulations available. That is to say, we conceive that explicit and quantitative theoretical structures are useful in guiding the direction of experimental research and in suggesting the type of data which are needed.

In this paper we describe a model, based upon elementary concepts of mathematical set theory. This model provides one possible framework for analyzing problems in stimulus generalization and discrimination. Further, we shall show how this model generates the basic postulates of our previous work on acquisition and extinction (1), where the stimulus situation as defined by the experimenter was assumed constant.

Stated in the simplest terms, generalization is the phenomenon in which an increase in strength of a response learned in one stimulus situation implies an increase in strength of response in a somewhat different stimulus situation. When this occurs, the two situations are said to be similar. Although there are several intuitive notions as to what is meant by "similarity," one usually means the properties which give rise to generalization. We see no alternative to using the amount of generalization as an operational definition of degree of "similarity." In the model, however, we shall give another definition of the degree of similarity, but this definition will be entirely consistent with the above-mentioned operational definition.

We also wish to clarify what we mean by stimulus discrimination. In one sense of the term, all learning is a process of discrimination. Our usage of the term is a more restricted one, however. We refer specifically to the process by which an animal learns to make response A in one stimulus situation and response B (or response A with different "strength") in a different stimulus situation. We are not at the moment concerned with, for example, the process by which an animal learns to discriminate between

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various possible responses in a fixed stimulus situation.

As prototypes of the more general problems of stimulus generalization and discrimination, we shall consider the following two kinds of experiments:

(i) An animal is trained to make a particular response, by the usual reinforcement procedure, in an experimentally defined stimulus situation. At the end of training, the response has a certain strength or probability of occurrence. The animal is then "tested" in a new stimulus situation similar to the training one and in which the same response, insofar as it is experimentally defined, is possible. One then asks about the strength or probability of occurrence of the response in this new stimulus situation and how it depends on the degree of similarity of the new situation to the old stimulus situation.

(ii) An animal is presented alternately with two stimulus situations which are similar. In one, an experimentally defined response is rewarded, and in the other that response is either not rewarded or rewarded less than in the first. Through the process of generalization, the effects of rewards and non-rewards in one stimulus situation influence the response strength in the other, but eventually the animal learns to respond in one but not in the other, or at least to respond with different probabilities (rates or strengths). One then asks how the probability of the response in each situation varies with the number of training trials, with the degree of similarity of the two situations, and with the amount of reward.

We do not consider that these two kinds of experiments come close to exhausting the problems classified under the heading of generalization and discrimination, but we do believe that they are fundamental. Thus, the model to be described has been designed to permit analysis of these experiments. In the next section we will present the major features of the model, and in later sections we shall apply it to the above described experiments.

THE MODEL

We shall employ some of the elementary notions of mathematical set theory to define our model. A particular stimulus situation, such as an experimental box with specific properties (geometrical, optical, acoustical, etc.) is regarded as separate and distinct from the rest of the universe. Thus, we shall denote this situation by a set of stimuli which is part of the entire universe of stimuli. The elements of this set are undefined and we place no restriction on their number. This lack of definition of the stimulus elements does not give rise to any serious difficulties since our final results involve neither properties of individual elements nor numbers of such elements. We next introduce the notion of the *measure* of a set. If the set consists of a finite number of elements, we may associate with each element a positive number to denote its "weight"; the measure of such a set is the sum of all these numbers. Intuitively, the weight associated with an element is the measure of the potential importance of that element in influencing the organism's behavior. More generally, we can define a density function over the set; the measure is the integral of that function over the set.

To bridge the gap between stimuli and responses, we shall borrow some of the basic notions of Estes (2). (The concept of reinforcement will play an integral role, however.) It is assumed that stimulus elements exist in one of two states as far as the organism involved is concerned; since the elements are undefined, these states do not require definition but merely need labelling. However, we shall speak of elements which are in one state as being "conditioned" to the

response, and of elements in the other state as being "non-conditioned."

On a particular trial or occurrence of a response in the learning process, it is conceived that an organism perceives a sub-set of the total stimuli available. It is postulated that the probability of occurrence of the response in a given time interval is equal to the measure of the elements in the sub-set which had been previously conditioned, divided by the measure of the entire sub-set. Speaking roughly, the probability is the ratio of the importance of the conditioned elements perceived to the importance of all the elements perceived. It is further assumed that the sub-set perceived is conditioned to the response if that response is rewarded.

The situation is illustrated in Fig. 1. It would be wrong to suppose that the conditioned and non-conditioned elements are spatially separated in the actual situation as Fig. 1 might suggest; the conditioned elements are spread out smoothly among the non-conditioned ones. In set-theoretic notation, we then have for the probability of occurrence of the response

$$p = \frac{m(X \cap C)}{m(X)}, \qquad (1)$$

where $m(\ )$ denotes the measure of any set or sub-set named between the parentheses, and where $X \cap C$ indicates the intersection of X and C (also called set-product, meet, or overlap of X and C). We then make an assumption of equal proportions in the measures so that

$$p = \frac{m(X \cap C)}{m(X)} = \frac{m(C)}{m(S)}. \qquad (2)$$

Heuristically, this assumption of equal proportions can arise from a fluid model. Suppose that the total situation is represented by a vessel containing an ideal fluid which is a

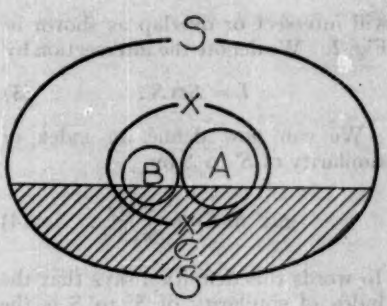

FIG. 1. Set diagram of the single stimulus situation S with the various sub-sets involved in a particular trial. C is the sub-set of elements previously conditioned, X the sub-set of S perceived on the trial. The sub-sets A and B are defined in the text.

mixture of two substances which do not chemically interact but are completely miscible. For discussion let the substances be water and alcohol and assume, contrary to fact, that the volume of the mixture is equal to the sum of the partial volumes. The volume of the water corresponds to the measure of the sub-set of non-conditioned stimuli, $S - C$ (total set minus the conditioned set), and the volume of the alcohol corresponds to the measure of the sub-set C of conditioned stimuli. The sub-set X corresponds to a thimbleful of the mixture and of course if the fluids are well mixed, the volumetric fraction of alcohol in a thimbleful will be much the same as that in the whole vessel. Thus the fraction of measure of conditioned stimuli in X will be equal to the fraction in the whole set S, as expressed by equation (2). Our definition of p is essentially that of Estes (2) except that where he speaks of number of elements, we speak of the measure of the elements.

We next consider another stimulus situation which we denote by a set S'. In general this new set S' will not be disjunct from the set S, *i.e.*, S and S'

will intersect or overlap as shown in Fig. 2. We denote the intersection by

$$I = S \cap S'. \tag{3}$$

We can now define an index of similarity of S' to S by

$$\eta(S' \text{ to } S) = \frac{m(I)}{m(S')}. \tag{4}$$

In words this definition says that the index of similarity of S' to S is the measure of their intersection divided by the measure of the set S'. (Our notation makes clear that we have made a tacit assumption that the measure of an element or set of elements is independent of the set in which it is measured.) Definition (4) also gives the index of similarity of S to S' as

$$\eta(S \text{ to } S') = \frac{m(I)}{m(S)} = \frac{m(S')}{m(S)}\,\eta(S' \text{ to } S). \tag{5}$$

From this last equation it is clear that the similarity of S' to S may not be the same as the similarity of S to S'. In fact, if the measure of the intersection is not zero, the two indices are equal only if the measures of S and S' are equal. It seems regrettable that similarity, by our definition, is non-symmetric. However, we do not care to make the general assumption that (a) the measures of all situations are equal and at the same time make the assumption that (b) measures of an element or set of elements is the same in each situation in which it appears. For then the importance of a set of elements, say a light bulb, would have to be the same in a small situation, say a $2' \times 2' \times 2'$ box, as in a large situation, say a ballroom. Further this pair of assumptions, (a) and (b), leads to conceptual difficulties.

THE GENERALIZATION PROBLEM

We are now in a position to say something about the first experimental problem described in the Introduction. An animal is trained to make a response in one stimulus situation and then his response strength is measured in a similar situation. After the animal has been trained in the first situation whose elements form the set S, a sub-set C of S will have been conditioned to the response as shown in Fig. 2. But part of the sub-set C is also contained in the second situation whose elements form the set S'; we denote this part by $C \cap S'$.

From the discussion preceding equations (1) and (2), we can easily see that the probability of the response occurring in S' is

$$p' = \frac{m(C \cap S')}{m(S')}. \tag{6}$$

We now use the assumption of equal proportions so that

$$\frac{m(C \cap S')}{m(I)} = \frac{m(C \cap I)}{m(I)} = \frac{m(C)}{m(S)}. \tag{7}$$

The first equality in this equation follows from the fact that the only part of C which is in S' is in the inter-

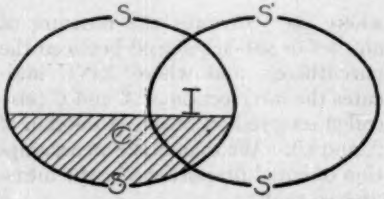

FIG. 2. Diagram of two similar stimulus situations after conditioning in one of them. The situation in which training occurred is denoted by the set S; the sub-set C of S represents the portion of S which was conditioned to the response. The new stimulus situation in which the response strength is to be measured is represented by the set S', and the intersection of S' and S is denoted by I.

section I as shown in Fig. 2. The second equality in equation (7) is an application of our assumption that the measure of C is uniformly distributed over S and so the intersection contains the same fraction of measure of C as does the entire set S.

If now we combine equations (6) and (7), we obtain

$$p' = \frac{m(I)}{m(S')} \cdot \frac{m(C)}{m(S)}. \qquad (8)$$

From equation (4) we note that the first ratio in equation (8) is the index of similarity of S' to S, while from equation (2) we observe that the second ratio in equation (8) is merely the probability p of the response in S. Hence

$$p' = \eta(S' \text{ to } S)p. \qquad (9)$$

Equation (9) now provides us with the necessary operational definition of the index of similarity, $\eta(S' \text{ to } S)$, of the set S' to the set S. The probabilities p and p' of the response in S and S', respectively, can be measured either directly or through measurements of latent time or rate of responding (1). Therefore, with equation (9), we have an operational way of determining the index of similarity.

As a direct consequence of our assumption of equal proportions, we can draw the following general conclusion. *Any change made in a stimulus situation where a response was conditioned will reduce the probability of occurrence of that response, provided the change does not introduce stimuli which had been previously conditioned to that response.* This conclusion follows from equation (9) and the fact that we have defined our similarity index in such a way that it is never greater than unity.

A word needs to be said about the correspondence between our result and the experimental results such as those of Hovland (3). Our model predicts nothing about the relation of the index of similarity defined above to such physical dimensions as light or sound intensity, frequency, etc. In fact, our model suggests that no such general relation is possible, *i.e.*, that any sensible measure of similarity is very much organism determined. Therefore, from the point of view of our model, experiments such as those of Hovland serve only as a clear demonstration that stimulus generalization exists. In addition, of course, such experiments provide empirical relations, characteristic of the organism studied, between the proposed index of similarity and various physical dimensions, but these relations are outside the scope of our model.

We conclude, therefore, that our model up to this point has made no quantitative predictions about the shape of generalization gradients which can be compared with experiment. Nevertheless, the preceding analysis of generalization does provide us with a framework to discuss experiments on stimulus discrimination. In the following sections we shall extend our model so as to permit analysis of such experiments.

THE REINFORCEMENT AND EXTINCTION OPERATORS

In this section we develop some results that will be used later and show that the model of the present paper generates postulates used in our previous paper (1). We shall examine the step-wise change in probability of a response in a single stimulus situation S. We generalize the notions already presented as follows: Previous to a particular trial or occurrence of the response, a sub-set C of S will have been conditioned. On the trial in question a sub-set X of S will be perceived as shown in Fig. 1. According to our previous assumptions,

the probability of the response is

$$p = \frac{m(X \cap C)}{m(X)} = \frac{m(C)}{m(S)}. \quad (10)$$

We now assume that a sub-set A of X will be conditioned to the response as a result of the reward given and that the measure of A will depend on the amount of reward, on the strength of motivation, etc. We further assume that another sub-set B of X will become non-conditioned as a result of the work required in making the response. For simplicity we assume that A and B are disjunct. (The error resulting from this last assumption can be shown to be small if the measures of A and B are small compared to that of S.)

We extend our assumption of equal proportions so that we have

$$\frac{m(A \cap C)}{m(A)} = \frac{m(B \cap C)}{m(B)} = \frac{m(C)}{m(S)}. \quad (11)$$

Now at the end of the trial being considered, sub-set A is part of the new conditional sub-set while sub-set B is part of the new non-conditioned subset. Thus, the change in the measure of C is

$$\begin{aligned} \Delta m(C) &= [m(A) - m(A \cap C)] \\ &\quad - m(B \cap C) \quad (12) \\ &= m(A)(1 - p) - m(B)p. \end{aligned}$$

This last form of writing equation (12) results from the equalities given in equations (10) and (11). If we then let

$$a = \frac{m(A)}{m(S)}, \quad b = \frac{m(B)}{m(S)}, \quad (13)$$

and divide equation (12) through by $m(S)$, we have finally for the change in probability:

$$\Delta p = \frac{\Delta m(C)}{m(S)} = a(1 - p) - bp. \quad (14)$$

We thus define a mathematical operator Q which when applied to p gives a new value of probability Qp effective at the start of the next trial:

$$Qp = p + a(1 - p) - bp. \quad (15)$$

This operator is identical to the general operator postulated in our model for acquisition and extinction in a fixed stimulus situation (1). Hence, the set-theoretic model we have presented generates the basic postulates of our previous model which we applied to other types of learning problems (1). When the operator Q is applied n times to an initial probability p_0, we obtain

$$Q^n p_0 = p_n = p_\infty - (p_\infty - p_0)g^n, \quad (16)$$

where $p_\infty = a/(a+b)$ and $g = 1 - a - b$.

In the next section we shall apply these results to the experiment on stimulus discrimination described in the Introduction.

THE DISCRIMINATION PROBLEM

We are now in a position to treat the second experimental problem described in the Introduction. An animal is presented alternately with two stimulus situations S and S' which are similar, *i.e.*, which have a non-zero

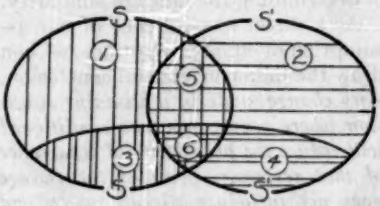

FIG. 3. Set diagram for discrimination training in two similar stimulus situations, S and S'. The various disjunct sub-sets are numbered. Set S includes 1, 3, 5, and 6; S' includes 2, 4, 5, and 6. The intersection I is denoted by 5 and 6. T, the complement of I in S, is shown by 1 and 3; T', the complement of I in S', is shown by 2 and 4. C, the conditioned sub-set in S, is represented by 3 and 6, while the conditioned sub-set in S', is represented by 4 and 6. T_e is denoted by 3, T_e' by 4, and I_e by 6.

intersection. The rewards which follow occurrences of the response are different for the two situations, and we are interested in how the response strengths vary with training. At any point in the process, sub-sets of S and S' will be conditioned to the response as shown in Fig. 3. We shall distinguish between that part of S which is also in S' and that part which is not by letting $I = S \cap S'$ and $T = S - (S \cap S') = S - I$. We also distinguish between the part of the conditioned sub-set C of S which is in I and that which is in T, by letting $I_c = C \cap I$ and $T_c = C - (C \cap I) = T \cap C$. The probability of the response in S is

$$p = \frac{m(C)}{m(S)} = \frac{m(T_c) + m(I_c)}{m(S)}. \quad (17)$$

Then we let

$$\alpha = \frac{m(T_c)}{m(T)}, \quad (18) \qquad \beta = \frac{m(I_c)}{m(I)}, \quad (19)$$

and, abbreviating $\eta(S \text{ to } S')$ with η, we may write (17) in the form

$$p = \alpha(1 - \eta) + \beta\eta. \quad (20)$$

We write the probability of the response in this form because we shall soon argue that the index η varies during discrimination training. First, however, we shall investigate the variation of α and β with the number of training trials. From the definitions of α and β, equations (18) and (19), we see that these variables are very much like our probability p of equation (17) except that they refer to sub-sets of S rather than to the entire set. By strict analogy with the arguments in the last section, we conclude that

$$\alpha_n = Q^n\alpha_0 = \alpha_\infty - (\alpha_\infty - \alpha_0)g^n, \quad (21)$$

where $\alpha_\infty = a/(a+b)$ and $g = 1-a-b$. Now, β, the fraction of conditioned stimuli in the intersection I, changes with each presentation of S' as well as of S. Thus, for each presentation of S, we must operate on β twice, once by our operator Q which describes the effect of the environmental events in S, and once by an analogous operator Q' which describes the effect of the events in S'. Hence, it may be shown that

$$\beta_n = (Q'Q)^n\beta_0 = \beta_\infty - (\beta_\infty - \beta_0)f^n, \quad (22)$$

where

$$\beta_\infty = \frac{a' + a(1 - a' - b')}{\{a' + a(1 - a' - b') + b' + b(1 - a' - b')\}}, \quad (23)$$

and where $f = (1-a'-b')(1-a-b)$.

It should be stressed that we are assuming that the response occurs and is rewarded to the same degree on every presentation of S. The same statement, mutatis mutandis, applies to S'. Without this assumption, we are not justified in applying the operators Q and Q' for each presentation. The probability is then the probability that the response will occur in an interval of time, h. The operational measure of this probability is the mean latent time, which according to the response model discussed earlier varies inversely as the probability (1).

We now have cleared the way for discussing the central feature of our model for discrimination problems. We conceive that *the measure of the intersection I of the two sets S and S' decreases as discrimination learning progresses.* This concept seems to make sense intuitively since the measure of any sub-set of stimuli indicates the importance of that sub-set in influencing behavior. If an animal is rewarded for a response in S but not rewarded for it in S', then the stimuli in I are unreliable for deciding whether or not to make the response. And it is just this ambiguity which causes the measure of the intersection to decrease with training. We shall describe this change by introducing a

"discrimination operator," denoted by D, which operates on the similarity index η each time the environmental event following the response changes from one type of event to another, *e.g.*, from reward to non-reward. In the present problem, we are considering alternate presentations of S and S' and thus alternate occurrences of the events associated with the operators Q and Q'. So if η_i is the ratio of the measure of I to that of S after the ith presentation of S, the ratio after the $(i+1)$th presentation is

$$\eta_{i+1} = D\eta_i. \tag{24}$$

Our next task is to postulate the form of the operator D.

We find that neither experimental data nor our intuition is of much help in guiding our choice of such a postulate. For mathematical simplicity we choose an operator which represents a linear transformation on η. Moreover, we wish to have an operator which always decreases η (or holds it fixed), but which will never lead to negative values of η. Therefore, we postulate that

$$D\eta = k\eta, \tag{25}$$

where k is a new parameter which is in the range between zero and 1. We then have

$$\eta_n = D^n\eta_0 = k^n\eta_0. \tag{26}$$

Combining equations (20), (21), (22), and (26), we have

$$\begin{aligned} p_n &= Q^n\alpha_0(1 - D^n\eta_0) + (Q'Q)^n\beta_0 D^n\eta_0 \\ &= [\alpha_\infty - (\alpha_\infty - \alpha_0)g^n](1 - k^n\eta_0) \\ &\quad + [\beta_\infty - (\beta_\infty - \beta_0)f^n]k^n\eta_0. \end{aligned} \tag{27}$$

This is our final expression for the variation of p_n, the probability of the response in situation S, as a function of the trial number n. This equation is composed of two major terms. The first term corresponds to the relative measures of the stimulus elements of S which are not in S' (the measure of T_e divided by the measure of S). The second term corresponds to the relative measure of the elements in the intersection of S and S' (the measure of I_e divided by the measure of S).

Because of the symmetry between S and S', we may write for the probability in S':

$$\begin{aligned} p_n' &= [\alpha_\infty' - (\alpha_\infty' - \alpha_0')g'^n](1 - k^n\eta_0') \\ &\quad + [\beta_\infty - (\beta_\infty - \beta_0)f^n]k^n\eta_0', \end{aligned} \tag{28}$$

where $\alpha_\infty' = a'/(a' + b')$, and $g' = 1 - a' - b'$, and where η_0' is the initial value of

$$\begin{aligned} \eta' &\equiv \eta(S' \text{ to } S) \\ &= \frac{m(I)}{m(S')} = \frac{m(S)}{m(S')}\eta. \end{aligned} \tag{29}$$

We shall now consider some special examples for which certain simplifying assumptions can be made.

(a) *No conditioning before discrimination training.* If no previous conditioning took place in either S or S', it seems reasonable to assume that the "operant" levels of performance in the two situations are the same. Moreover, in view of our assumptions of equal proportions, we may assume that *initially:*

$$\begin{aligned} \frac{m(C)}{m(S)} &= \frac{m(T_e)}{m(T)} = \frac{m(I_e)}{m(I)} \\ &= \frac{m(T_e')}{m(T')} = \frac{m(C')}{m(S')}. \end{aligned} \tag{30}$$

Hence, from equations (17), (18), and (19), we have $p_0 = \alpha_0 = \alpha_0' = \beta_0$. Moreover, inspection of equation (27) shows that, except when $k = 1$, we have $p_\infty = \alpha_\infty$, and in like manner from equation (28) for $k \neq 1$, we have $p_\infty' = \alpha_\infty'$. In Fig. 4 we have plotted equations (27) and (28) with the above assumptions. The values $a = 0.12$, $b = 0.03$, $p_0 = 0.05$, $\eta_0 = \eta_0' = 0.50$, $k = 0.95$ were chosen for these calculations. As can be seen, the proba-

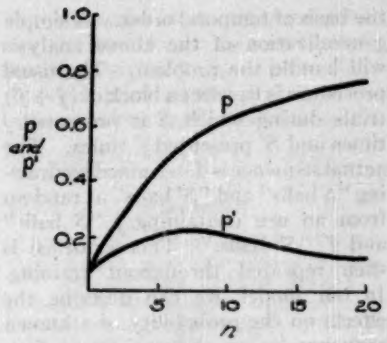

FIG. 4. Curves of probability, p (in S), and p' (in S'), versus trial number, n, for discrimination training without previous conditioning. It was assumed that the response was rewarded in S but not rewarded in S'. Equation (27), equation (28), and the values $p_0 = p_0' = 0.05$, $a = 0.12$, $a' = 0$, $b = b' = 0.03$, $\eta_0 = \eta_0' = 0.50$, and $k = 0.95$ were used.

bility of the response in S is a monotonically increasing, negatively accelerated function of the trial number, while the probability in S' first increases due to generalization, but then decreases to zero as the discrimination is learned. These curves describe the general sort of result obtained by Woodbury for auditory discrimination in dogs (4).

We have argued (1) that the mean latent time varies inversely as the probability. Thus in Fig. 5 we have plotted the reciprocals of p_n and p_n' given in Fig. 4. These curves exhibit the same general property of the experimental curves on running time of rats obtained by Raben (5).

(b) *Complete conditioning in S before discrimination training.* Another special case of interest is that in which the set S is completely conditioned to the response before the discrimination experiment is performed. In this case, $\alpha_0 = \beta_0 = p_0 = p_\infty$. In Fig. 6 we have plotted p_n and $1/p_n$ with these conditions and the values $p_\infty = 1$, $\beta_\infty = 0$, $\eta_0 = 0.80$, $k = 0.90$, and $f = 0.50$. The curve of $1/p$ versus n is similar in shape to the experimental latency curve obtained by Solomon (6) from a jumping experiment with rats.

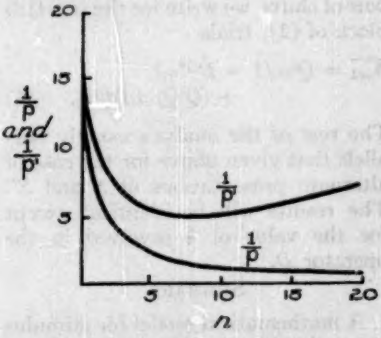

FIG. 5. Reciprocals of probability, p, of the response in S, and p', of the response in S', versus trial number, n, for discrimination training without previous conditioning. In the model described earlier (1), mean latent time is proportional to the reciprocal of probability. The curves were plotted from the values of probability shown in Fig. 4.

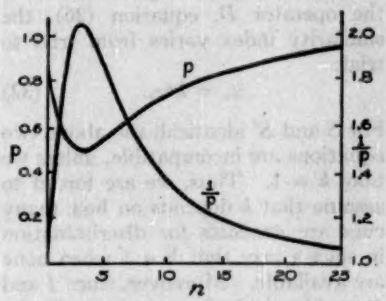

FIG. 6. Curves of probability, p, and its reciprocal versus trial number, n, for the case of complete conditioning in S before the discrimination training. Equation (27) with the values $p_\infty = 1$, $\beta_\infty = 0$, $\eta_0 = 0.80$, $k = 0.90$, and $f = 0.50$ were used.

(c) *Limiting case of S and S' identical.* Another limiting case of the kind of discrimination experiment being

considered here obtains when we make the two stimulus situations S and S' identical. The problem degenerates into one type of partial reinforcement where, for example, an animal is rewarded on every second trial in a fixed stimulus situation. The intersection I of S and S' is of course identical to both S and S'. Thus the measure of I must equal the measure of S. From equation (5), we have

$$\eta = \frac{m(I)}{m(S)} = 1, \tag{31}$$

while according to our postulate about the operator D, equation (26), the similarity index varies from trial to trial:

$$\eta_n = k^n \eta_0. \tag{32}$$

For S and S' identical, the above two equations are incompatible, unless we take $k = 1$. Thus, we are forced to assume that k depends on how many cues are *available* for discrimination in such a way that $k = 1$ when none are available. Moreover, since I and S are identical, the measure of T, the complement of I in S, must be zero. Since T_e is a sub-set of T, the measure of T_e must also be zero. Therefore, equations (17) and (19) give in place of equation (20)

$$p = \beta\eta. \tag{33}$$

But we have just argued that for S and S' identical, we have $\eta = 1$. Thus

$$p = \beta. \tag{34}$$

Equation (22) gives us then

$$p_n = (Q'Q)^n p_0 = p_\infty - (p_\infty - p_0)f^n. \tag{35}$$

This equation agrees with our previous result on partial reinforcement (1).

(d) *Irregular presentations of S and S'.* In most experiments, S and S' are not presented alternately, but in an irregular sequence so that the animal cannot learn to discriminate on the basis of temporal order. A simple generalization of the above analysis will handle the problem. The usual procedure is to select a block of $(j + j')$ trials during which S is presented j times and S' presented j' times. The actual sequence is determined by drawing "S balls" and "S' balls" at random from an urn containing j "S balls" and j' "S' balls." This sequence is then repeated throughout training. In our model, we can describe the effects on the probability of a known sequence by an appropriate application of our operators Q, Q', and D for presentations of S, presentations of S', and shifts from one to the other, respectively. A less cumbersome method provides a reasonable approximation: for each block of $(j + j')$ trials we describe an effective or expected new value of probability by applying Q to its operand j times, Q' to its operand j' times, and D to the index η a number of times determined by the mean number of shifts from S to S'. For the special case of $j = j'$, the mean number of shifts is j. Since previously, we applied D to η for each *pair* of shifts, we write for the $(i+1)$th block of $(2j)$ trials

$$\overline{p_{i+1}} = Q^j\alpha_i(1 - D^{j/2}\eta_i) + (Q'Q)^j\beta_i D^{j/2}\eta_i. \tag{36}$$

The rest of the analysis exactly parallels that given above for the case of alternate presentations of S and S'. The results will be identical except for the value of k involved in the operator D.

SUMMARY

A mathematical model for stimulus generalization and discrimination is described in terms of simple set-theoretic concepts. An index of similarity is defined in terms of the model but is related to measurements in generalization experiments. The mathematical operators for acquisition and extinc-

tion, discussed in an earlier paper (1), are derived from the set-theoretic model presented here. The model is finally applied to the analysis of experiments on stimulus discrimination.

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PERCEPTION, ASSOCIATION, AND REASONING IN ANIMAL FIXATIONS [1]

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Introduction

Maier has summarized the work he and his students have done in producing abnormal fixations in animals, defining abnormal fixation as "the persistence of an unadaptive response . . . when an adaptive one is possible" (6, p. 33). Mowrer has referred to the problem of explaining such behavior at the human level as "the absolutely central and most critical issue in clinical theory . . . why so-called neurotic behavior is at one and the same time *self-defeating and yet self-perpetuating*, instead of self-eliminating" (10, p. 434).

What answers have been offered to meet this issue? At the human level neurotic behavior has been explained by various personality theories, including the varieties of psychoanalytic theory reviewed by Mullahy (11). Attempts to explain repetitive, self-defeating behavior at the animal level are usually integrations of "learning theory and the neurotic paradox," as Mowrer (9) phrases it.

Theories of animal fixations may be pictured as forming a double-Y maze. The first fork is the dichotomy of those explanations which postulate a change in the motivational state of the animal, and those which do not. Along the former path is a second forking which Farber has described (1, pp. 113–114). This is the dichotomy of anxiety-reduction theory, espoused by Farber, Mowrer (8), and Miller (7), and Maier's frustration theory. The anxiety-reduction view is that the fixated animal's goal is to reduce anxiety, while Maier, on the other hand, offers the more extreme view that "frustration-instigated" behavior is "behavior without a goal." Both theories agree that sometimes during the experimental procedure the animal loses sight of its original goal of avoiding punishment.

Is this a necessary assumption? Must we choose between these two theories, the one positing that the goal changes to anxiety-reduction, the other that the goal disappears altogether? Or can we accept the animal's motivation as a constant throughout the experimental procedure, and assert that even the fixated animal has as its goal the avoidance of the locked window?

To assert this is to backtrack to the first Y of our maze, where we found this one path assuming a motivational change, and to take the other branch, in which motivation is assumed to be a constant, and the fixation is explained in terms of cognitive difficulty. This is a path suggested by Hilgard (2, pp. 340–341). Only if this cognitive alley ends in a cul-de-sac are we justified in postulating the more radical assumption that the animal's goal changes.

Procedure Used by Maier

Placed on the Lashley apparatus, the animal must jump from a small stand through either of two card-covered windows. If the correct card is struck, the animal lands

[1] This article grew out of discussions between Miss Victoria Lewicki, Mr. Paul Ellen, and myself. Without their stimulating suggestions, the paper could not have been written. After it had been completed, Dr. N. R. F. Maier was kind enough to read it and to criticize the presentation. This does not mean that either Miss Lewicki, Mr. Ellen, or Dr. Maier are in agreement with the ideas presented here.

on the feeding platform. If the incorrect card is struck, the animal receives a bump on the nose from the locked card and falls into a net below. If one window is consistently locked (position-reward problem), the animal learns to jump to the other one (position-reward response), regardless of which card covers it; if one card is consistently locked (symbol-reward problem), the animal learns to jump to the other one (symbol-reward response) regardless of which window it covers. In the insoluble problem the cards are locked randomly, and the animal eventually settles down to a stereotyped response, jumping consistently to one window or card.

Once the animal has successfully learned the position-reward response (or, in the insoluble problem, developed a position stereotype), it is given a soluble problem. If the animal, after being given the solvable problem, fails to abandon its position response in 200 trials, it has met the criterion of abnormal fixation.

Application of perception, association, and reasoning (PAR).

1. Perception: When animals go from a situation of 50 per cent random punishment (insoluble problem) to a situation of 50 per cent orderly punishment (symbol-reward problem), they must first perceive that the pattern of punishment has changed.

2. Association: In the symbol-reward problem, four associations are possible:

(a) Positive card with reward, on habitual side.
(b) Positive card with reward, on opposite side.
(c) Negative card with punishment, on habitual side.
(d) Negative card with punishment, on opposite side.

3. Reasoning: Although the rat may learn through association that the positive card will be rewarding and the negative card punishing when the cards are in the window to which the animal habitually jumps, this learning does not necessarily include the knowledge that the positive and negative cards will also reward and punish, respectively, when in the other window. The process by which the animal generalizes, hypothesizes, or infers the pattern of reward and punishment at the nonhabitual window is what is being termed reasoning, because the animal must put two separate experiences together if the proper significance is to be discovered (3, p. 336; 4, p. 375).

FINDINGS AND INTERPRETATIONS

1. *Comparison of Randomly-Punished with Rewarded Animals*

(a) Differential number of fixations. The basic experimental design upon which frustration theory is based is one in which a group of animals is presented with the no-solution problem, involving 50 per cent random punishment, while a control group is given a solvable problem. During the final 30 criterion runs in this situation, the control group, having solved the problem, is never punished. Both groups are then given a symbol-reward problem to solve. Maier summarizes the results of three experiments of this kind:

". . . of the 34 rats that developed position stereotypes in the no-solution problem only 10 (29.4 per cent) were able to abandon this response and adopt a symbol-reward response in 200 trials or less. However, among the 35 rats that acquired a position-reward response, 26 (74.3 per cent) were able to change to a symbol-reward response . . ." (6, p. 32).

Maier points out that the experimental group had the more frustrating experience and that this accounts for the group differences. However, the experimental group also had an entirely different problem to solve, one which was more difficult than the problem facing the control animals.

First, the problems differ with respect

to the factor of perception. The position-reward animals (control group) have been experiencing 100 per cent reward for the last 30 jumps. Suddenly they experience 50 per cent orderly punishment. The only discrimination they are required to make in order to realize that the objective situation has changed is between no punishment and 50 per cent punishment. On the other hand, the randomly-punished animals (experimental group), as Hilgard (2, pp. 303–304) points out, must perceive the more subtle change from 50 per cent random punishment to 50 per cent orderly punishment. Unless they are able to discriminate this difference, they have no reason to drop their position stereotype and to try the alternate window. The window had been tried in the past and found wanting. Why try it again?

Moreover, perceiving that the objective situation has somehow changed is not enough. Certain specific associations, which the control group need not make, must be built up by the experimental animals. This is the double association of positive card with reward and negative card with punishment. For the control animals a simple association of negative card with punishment is enough. This one association tells them that whereas it is still all right to jump to their habitual window when the positive card is there, they had better jump elsewhere when the negative card covers it. This "elsewhere" can only be the alternative window.

Finally, the experimental group must hypothesize or infer that the same pattern of reward and punishment which now holds at the habitual window will also hold at the other window. This may seem an obvious, even automatic, inference to the experimenter, to whom it might never occur to give orderly punishment at one window and random punishment at the other. It is an easy problem from the experimenter's point of view, not because of greater intelligence, but because he can peek behind the scenes and *perceive* the pattern which the animal (except in guidance, as we shall see later) must *reason* out.

The experimental group, in short, is faced with the more difficult problem. In order to master the symbol-reward situation, they must solve all three problems of PAR. Unless it can be demonstrated that the animal has done this, any conative theory of fixation is superfluous. Maier, in stating that the animal jumps to the habitual window because it *must*, in spite of learning, is implying that the animal has indeed solved all three problems. Is there any evidence that the fixated animal has solved any of the three?

There is. Maier's evidence for this is the next experimental finding to be discussed.

(b) Differential resistance to positive and negative cards. Maier offers evidence suggesting that the animal has solved the first two problems. This evidence consists in the fact that even fixated animals show more resistance to jumping whenever the negative card is in their habitual window.

Maier interprets this to mean that the animals have learned the new problem, but that because of the compulsive nature of the fixation, they are unable to behave in accordance with this learning. On the other hand, it may be that although all animals successfully associate the positive card with reward and the negative card with punishment when on the habitual side, some are unable to generalize to the alternative side.

This is the point Hilgard makes, and is the crucial point of this entire paper. To give a human analogy, similar to Hilgard's (2, pp. 340–341), we might suppose that a gambler has in the past found he could do no better than chance

either with dice or with horse-racing. Suddenly he finds that the situation has changed, and that he can now predict winners at the race track. The ability to predict in this situation would be little incentive for him to stop betting on the horses in favor of returning to dice. It is, in fact, to his advantage to stay where he is.

In many respects this is the position of the fixated animals. If they continue jumping to their habitual window, they can successfully predict the occurrence of reward and punishment, while their experience during the insoluble problem has taught them that if they revert to the other window, they are unable to predict.

This prediction may seem worthless, since the animal, like the gambler, has no control over the outcome. But the animal does have control over the amount of punishment it must endure from the locked window, just as the gambler can control the amount he stands to win or lose on a race. Because the animal has abortive jumps within its repertoire, it too is placing a bet. An abortive jump, in which the animal may jump so softly that it fails to reach the window, protects it against punishment upon encountering a locked window, but prevents it from gaining access through an unlocked window. If the animal should make an abortive jump at a time when the window is locked, it suffers less punishment. If it should make an abortive jump at a time when the window is unlocked, it suffers more punishment and gains less reward. Consequently it is to the rat's advantage to know when the window is locked. It can achieve this advantage only at the habitual window. There is no incentive to give this up.

Moreover, the animal may make an interpretation of the pattern of reward and punishment entirely different from that which a human would make, though perhaps not so different from that which man would have made in his early cultural development before magic was superseded by science. The fixated animal may even learn that jumping at the negative card makes the positive card appear.

In brief, then, the difference in the number of fixations occurring between control and experimental animals might be explained by the greater difficulty of one of the problems.

2. *The Non-Normal Distribution*

In an ordinary learning problem one expects the scores made by the total group to form a normal distribution, reflecting individual differences in the ability measured. But when giving control animals the symbol-reward problem, Maier finds that they split into two groups, those who learn the problem fairly rapidly (within 100 trials), and those who never learn it (6, p. 34). Maier interprets this to mean that the frustration which the rats endured caused some of them to switch from motivated to frustrated behavior.

However, if a minimum amount of PAR abilities is necessary in order to solve the symbol-reward problem, and if some animals have less than this minimum, then the dichotomous distribution is explainable without the hypothesis of a motivational change.

Moreover, there are individual differences, not only in PAR abilities, but also in variability: From the time of initial training on the Lashley apparatus, some animals resist the experimenter's efforts to get them to jump to their non-preferred window. One can expect a few animals who are too persistent in their habits to change when the situation demands it. Since this persistency appears in the earliest part of their training, it is apparently innate and not

the result of any frustrating experiences the animals have encountered.

Individual differences in the ability to generalize and to adapt may thus account for the non-normal distribution which Maier obtains.

3. *The Differential Effect of Rewarding the Preferred and the Non-Preferred Window*

From the time the animal begins its training on the Lashley apparatus, it shows a preference for one of the two windows, often so strong that unless forced to do so the animal will never try an alternate window. When the experimenter trains the control group to a position-reward response, he must decide if he will train the animals to their preferred side or to the non-preferred.

If he decides to train them to their preferred window, little or no training in discrimination is required. Both windows are left unlocked; the animal jumps to one; thereafter that window continues to be unlocked, and the animal will usually jump there consistently.

For the animals whose correct window is opposite their preference, both windows are locked; the animal jumps to one, and thereafter that window continues to be locked. Thus, these animals are trained to the non-preferred window by making them experience their preferred window as 100 per cent punitive.

Ordinarily we should expect that animals who have been forced to jump to a non-preferred window would find it easier to surrender this in favor of a new response than would animals who have been jumping with equal frequency to their preferred window. However, Maier has shown that

> ". . . animals trained in position-reward responses that correspond to their natural preference are more able thereafter to acquire a symbol-reward response than animals trained to a position-reward response opposed to their expressed natural preference" (6, p. 35).

Maier explains this in terms of the greater frustration endured by the non-preference group. However, the significance of this greater frustration or punishment may lie in its effect, not upon the motivational state of the animals, but upon their understanding of the situation in which they find themselves.

Since the preference animals never experience the alternate window as punitive, when their habitual window begins punishing them each time the negative card is in it, they naturally attempt to avoid that card by jumping elsewhere, and this can be only to the other window.

The non-preference group also finds the habitual window punishing them whenever the negative card is in it. If they stay where they are, they too will receive 50 per cent punishment. But they have previously learned that if they try the alternate window, their preferred window, they will fare even worse. Not knowing the mind of man, and unable from their rats'-eye view to perceive the total pattern of reward and punishment, they see no reason for supposing that, when the habitual window changes from purely rewarding to frequently punishing, the other window changes from purely punishing to frequently rewarding.

In other words, the difference between preference and non-preference group may be that one has no cognitive map of the alternate window, so that it remains open for exploration, while the other has a very clear map of that window as leading only to punishment, and hence as something to be avoided.

4. *Comparison of Symbol-Reward and Position-Reward Problems*

In each experiment mentioned thus far, the animal with a position response

had to learn a symbol response. The habitual window is locked whenever the negative card is in it; so long as the rat retains its habitual response, it experiences 50 per cent orderly punishment. In other experiments, however, the animal is given a new position response to learn. The habitual window is permanently locked, and the rat experiences 100 per cent punishment so long as it keeps the old position habit.

In comparing the behavior of animals on each of these problems, Maier notes that:

"punishment on 100 per cent of the trials causes fewer animals to abandon a response than does punishment on 50 per cent of the trials. . . . [But] 100 per cent punishment makes . . . for more rapid learning than does 50 per cent punishment.

"When these apparently contradictory results are combined we may say that 100 per cent punishment makes either for fairly rapid response alteration or no alteration at all, whereas 50 per cent punishment makes for slower but surer learning" (6, pp. 37–38).

There are two results to be explained here:

1. More animals fixate in the 100 per cent punishment situation.

2. When learning occurs at all, it occurs more rapidly in the 100 per cent punishment situation.

Maier points out that the second result is consistent with prevalent learning theories, and applies to the animals who continue to be motivated. He implies that the first result is inconsistent with learning theory, but consistent with frustration theory, and applies to the animals who change to frustrated behavior.

Both results, however, can be explained without postulating a motivational change:

1. Why do fewer animals solve the 100 per cent punishment problem? What is required of them? First, they must perceive the change from 50 per cent to 100 per cent punishment. Since in the former situation they may experience as many as three consecutive punished trials in a given day's run of 10 jumps, their perceptual problem is one of discriminating between three and more-than-three. Secondly, with respect to associative learning the animals are in an impossible situation, for they are given no opportunity to associate reward with the correct response; that is to say, they cannot learn that the alternative window will be rewarding until after they jump to it, so they never experience a pattern of reward and punishment. Since the rat cannot be expected to associate or learn what it does not experience, these animals are not in a learning situation at all. They are being called on to perform, not learn. Maier himself implies that these animals are not in a learning situation when he refers to their task in terms of "response alteration," and to the symbol-reward group in terms of "learning."

These symbol-reward animals, experiencing 50 per cent punishment, are given the opportunity to associate the positive card with reward and the negative card with punishment each time the card appears in the habitual window. They need make no alteration in their habitual mode of jumping in order to make this double association. Faced with a comparatively simple problem, more of them solve it.

2. Why, then, are they slower in changing their behavior? It is because time is required, first, to perceive the subtle change from 50 per cent random punishment to 50 per cent orderly, and secondly, to build up the double association.

It is as though we were to ask one group of students to memorize nonsense syllables, and to ask a similar group to solve a problem in calculus, *e.g.*, to differentiate x^2. Since those comparatively

few students with the ability to solve calculus problems would quickly answer "$2x$," while the majority without this ability would never solve the problem, this would make either for fairly rapid response or for none at all. On the other hand, the first problem would make for slower but surer learning. Similarly, most of the symbol-reward group solve the problem, but require time, while those few position-response animals who solve their problem do so quickly.

5. *Guidance*

Maier discusses several ineffective attempts to break fixations, including the use of shock, vacation, and a variety of experiences on the Lashley apparatus. Since none of these procedures changes the nature of the problem confronting the animal, it is consistent with PAR theory that they were ineffective. He then describes the method of guidance, in which the animal is *forced* to jump to the correct window, and its effect upon fixations:

". . . 23 rats with position fixations could not execute a symbol-reward discrimination response in 200 trials but shifted to a perfect discrimination after a few trials of guidance. . . .

"Only 2 out of 28 guided animals . . . learned the symbol-reward discrimination response by guidance alone . . ." (6, pp. 53–57).

Again there are two separate points to be explained:

1. How is guidance effective in breaking fixations which are otherwise so persistent?

2. Why is guidance usually ineffective by itself in teaching a new response?

Maier explains these two points by suggesting that guidance does not teach the animal anything, but simply tears it away from the fixated response (6, pp. 54–58). An alternative explanation is that guidance enables the animal to experience what it cannot infer.

1. Why does the fixated animal drop its fixation under the influence of guidance? The three basic assumptions mentioned at the beginning of this paper include four associations which the animal needs in order to understand the pattern of reward and punishment prevailing in the symbol-reward problem. According to PAR theory, the fixated animal builds up two of these associations by means of direct experience, and fixation is the result of failure to infer the remaining two. Guidance makes it unnecessary for the animal to make one of these two remaining associations, namely, the association of positive card with reward at the non-habitual window. Once the animal has learned through the direct experience of which guidance consists that it can avoid punishment by jumping to the alternative window whenever the negative card is in the habitual window, it has sufficient understanding of the situation to break the fixation. The animal has not, however, solved the symbol-reward problem.

2. Why not? Why does the rat continue to make errors? What more need it know? The answer, of course, is that it has not yet learned that the negative card will be punitive at the alternative window. The animal cannot learn this while jumping to the habitual window, nor can it learn this from the guided trials. Only trial and error can give this fourth and final association. Once the association is made, the animal can put the parts together into a completed 4-piece jigsaw puzzle, to form a cognitive map of the terrain.

6. *The Effect of Duration of Punishment upon Fixations*

In each experiment referred to so far, amount of frustration and difficulty of the problem varied concomitantly, so that those animals who were forced

to endure more punishment were also given the more difficult problem to solve. In an experiment which was obviously not designed to test PAR theory, Maier and Feldman (5) held one factor constant for two groups and varied the other:

"Litters were divided into three groups; the first group was subjected to the insoluble problem for 8 days, the second for 16 days, and the third for 24 days. . . . As controls, three other groups, made up of divided litters, were trained by reward and punishment to form position responses. One group received 8 days of practice, the second 16 days of practice, and the third 24 days of practice. . . .

"After the position responses were established in both the experimental and the control groups, the problem was changed so that the animals were required to learn the symbol-reward discrimination problem. . . . Guidance and trial and error were alternated. [The 8, 16, and 24 day reward groups required 22.2, 20.2, and 19.8 trials (average), respectively, to abandon the position response.]

"None of the differences in average scores is significant, but it is of interest to note that increasing the number of days of training has not increased the strength of the position-reward response. [The 8, 16, and 24 day randomly-punished groups required 30.1, 49.0, and 44.4 trials (average), respectively, to abandon the position stereotypes.]

"Both the 16-day and the 24-day groups are [significantly] different from the 8-day group. . . . All groups are also significantly different from the groups trained in position rewards; . . ." (6, pp. 60–62).

We can now make comparisons within control and experimental groups, as well as between them:

1. The control groups all solved the problem with equal facility. Since these three groups had similar problems to solve, the result is in accordance with PAR theory.

2. The experimental animals required more trials in which to solve the problem than did the control animals. Since this latter group had the easier problem, this too is in accordance with PAR theory.

3. That there is no difference between the 16- and the 24-day experimental groups is what we would expect. But that these two groups would differ significantly from the 8-day animals is not what PAR theory would predict. In what way is the latter's problem different from that of the other randomly-punished animals?

First, with respect to the experience at the habitual window, this group experienced less punishment than did the others, and should have been less rather than more rapid in giving up the window; or, if frequency of punishment is irrelevant, the three groups should have been equal.

Is there any way, however, in which the greater punishment endured at the habitual window by the 16- and 24-day animals might have made the problem a more difficult one for them? While it seems possible that the punishment had an emotional effect upon the animals, so that there was a loss in the ability to perceive, associate, or reason, evidence is against this:

". . . [In] tests made on the Maier three-table reasoning problem . . . 14 rats with position fixations made an average of 3.3 incorrect responses in 12 tests. . . . Six nonfixated rats made an average of 3.2 errors. . . .

"Further, an animal may learn a symbol-reward discrimination for walking and retain the position fixation for jumping" (6, pp. 47–48).

That the fixated animals could both reason and learn satisfactorily suggests that the punishment suffered during the insoluble problem did not affect PAR abilities. The use of control animals suggests that the greater frequency of reward experienced by the 16- and 24-day groups is also irrelevant.

Apparently the briefer experience of the 8-day animals at the habitual window sheds no light on their greater ability to solve the symbol-reward problem. Let us turn, then, to the respective experiences of the animals at the non-habitual window.

If the 16-day group had experienced this window as punitive more frequently than the 8-day animals, they would be more reluctant to jump to it. However, since animals in the insoluble problem seldom jump to the non-habitual window after the 5th or 6th day, all three groups probably experienced this window equally often. Moreover, the 8-day group had experienced the window as punitive only a few days before being presented with the symbol-reward problem, while for the 24-day group almost 20 days had elapsed. If the principle of recency is relevant here, the 8-day group should have shown greater reluctance to jumping at the non-habitual window than the others.

Both on the basis of the lesser punishment endured at the habitual window, and on the basis of the more recent punishment endured at the non-habitual window, PAR theory would predict that the 8-day group would find it more difficult than the other animals to surrender their position response in favor of a symbol response. Since the experimental results are directly opposed to this prediction, we are left with this question: Why does the strength of a position-stereotype response increase as the animal spends more time in the insoluble problem, while the strength of a position-reward response does not increase with time spent in the reward situation?

7. *The Open-Window Situation*

One way of equating the difficulty of the problems facing control and experimental groups is to increase the difficulty for the control animals, as in the Maier-Feldman study, if we use the 8-day punished animals as controls. Another way is to decrease the difficulty of the problem facing the experimental animals. This can be done by removing the card from the non-habitual window. Maier offers two photographs illustrating the behavior of a fixated animal when confronted with an open window on its non-habitual side:

> "we see the rat observing the food in the open window. The behavior of the animal on these occasions is one of sniffing and reaching out toward the open window. . . . Yet, invariably, the rat finally turns and jumps in conformity with its position fixation . . ." (6, between pp. 114 and 115).

The crucial point of this paper has been that the fixated animal does not know what awaits it at the non-habitual window. This is not true in the open-window situation. The card is removed, just as it was during preliminary training, when the animal learned that without the card the window was always rewarding, never punishing. Food is in sight, well within the animal's visual range, at the same distance, in fact, as the cards have been. The sniffing which the rat does shows that the food is also within olfactory range.

The animal is thus given the answer to the problem we have been postulating it is unable to solve. It need make no inference now, nor do any more than follow its nose. Yet it fails to do this. Why?

Perhaps the tremendous amount of punishment the animal has received has changed the interpretation it makes even of direct sensory stimuli. By first cajoling the rat into jumping, with the presentation of consistent reward, and then rigging up a punitive situation from which it could not escape, the experimenter tricked the animal. The rat may now have developed a generally suspicious attitude toward the environ-

ment, seeing it as threatening despite any surface appearance.

The thesis that the animal no longer perceives the same meaning in objects as it once did has evidence against it. On the three-table reasoning problem referred to above, the animal perceives as non-fixated animals do. Even on the Lashley apparatus, the animal responds to rewarding stimuli in the conventional manner, provided it can walk rather than jump.

We are again left with an unanswered question: Why does the animal continue jumping at its fixated window when there is apparently no longer any problem involving perception, association, or reasoning?

CONCLUSION

While much of the data which Maier offers as evidence for frustration theory can be otherwise explained, the two experimental results remaining unaccounted for are relevant to two questions which any theory of fixation must answer:

1. In general, why do fixations occur?
2. In a given experiment, why do more fixations occur in one group than in another?

Any theory of cognitive difficulty must answer that fixations occur because the animal is unable to solve the problem, and that more fixations occur in the group with the more difficult problem. The open-window phenomenon and the Maier-Feldman study refute, respectively, each of these explanations. When faced with an open window, the animal has no problem to solve, and no fixations should occur. In the Maier-Feldman experiment, the 16-day randomly-punished animals faced the same problem as did the 8-day group, and an equal number of fixations should have occurred.

It seems unlikely that some future application will reconcile these results with what a theory of cognitive difficulty would predict. As a matter of fact, it may have seemed that liberties have been taken in order to make a theory fit the data. This was necessary if we were to make sure that fixations could not be explained in cognitive terms. Before accepting new concepts —and I think both anxiety-reduction and frustration-instigated behavior are new concepts—it is worthwhile to make sure the old ones are inadequate for the purpose. In order to do this, it may be necessary to put old concepts, like PAR, to new uses.

If after granting our cognitive theory every assumption, we find it inadequate, where do we go next?

In the introduction the various theories were described as forming a double-Y maze. At the first fork, we took the cognitive alley, postulating no motivational change in the animal, and found it promising, but a cul-de-sac. It behooves us now, as it would any intelligent animal, to retrace our steps and to try the conative alley, postulating a motivational change.

It too contains two branches, frustration and anxiety-reduction. We explored the cognitive alley first because it seemed the more conservative of the two. By the same criterion the anxiety-reduction alley should be explored next. Only if postulating that the animal exchanges one goal for another leads to a blank wall, can we seriously consider the possibility that the animal exchanges his goal for no goal at all, that the fixated response is "behavior without a goal."

SUMMARY

By making certain assumptions about the application of the principles of perception, association, and reasoning (PAR) to the experimental design used by Maier in producing animal fixations, we were able to account for much of his

data, but found two results which we were unable to explain:

1. The strength of a position-stereotype response increases as the animal spends more time in the insoluble problem.

2. If the non-habitual window is opened, exposing the food, a fixated animal continues to jump at the habitual window.

It seems unlikely that PAR can account for these two phenomena, since cognitive difficulty for control and experimental groups is equated. In order to explain them satisfactorily, we must concern ourselves with the motivational factor.

The two current theories which take this factor into account in attempting to explain fixations are anxiety-reduction theory, in which the new motive becomes one of reducing anxiety, and frustration theory, in which there is no motive for the behavior. Logically, anxiety-reduction theory should be explored next. Only if this proves to be a blind alley should we seriously consider the possibility that the fixations are "behavior without a goal."

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CAN THE ANXIETY–REDUCTION THEORY EXPLAIN ABNORMAL FIXATIONS?

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In a recent review of Maier's book on *Frustration*, Hilgard (2) pointed out that the theory of anxiety-reduction, as primarily developed by Mowrer, Miller, and others (1, 10, 12, 13, 14, and 15), was not adequately treated as an alternative to the theory set forth. McClelland (11) has made a similar observation, but goes so far as to state that the anxiety-reduction theory would adequately explain the results. Interestingly enough, Mowrer's (16) own review does not suggest that the anxiety-reduction concept has been slighted.

Since the most relevant investigations on anxiety-reduction were published after Maier's book was in press, it was not possible to include them in the discussion. Those that were published seemed, at the time, not to be relevant both because frustration was not stressed, and because the writers on anxiety-reduction made no suggestion that there was a relationship between their work and the work of Maier and his students. However, some of the recent publications on anxiety-reduction seem to be relevant and justify Hilgard's and McClelland's observation that similar results are being explained by different concepts. Whether or not the writers on anxiety-reduction would agree to this observation is difficult to say. Certainly the exponents of the theory have made no claim that their theory can explain the basic facts of abnormal fixations. Nevertheless, in the light of opinions expressed by Hilgard and McClelland, and our own feeling that the recently published work on anxiety-reduction is relevant, we feel justified in supplementing the frustration theory with a consideration of the evidence leading to the anxiety-reduction theory.

The anxiety-reduction theory is of particular importance because it is consistent with the view that abnormal behavior can be reduced to learning concepts, and, at the same time, postulates a need which formerly either was not stressed or was entirely overlooked. Anxiety-reduction represents a need that the organism might develop in a stressful situation and it might be quite unrelated to the need that the experimenter may have regarded as the dominant one. Thus, an experimenter might design his experiment on the basis of a primary need and the animal may be behaving on the basis of a secondary need. This difference between the experimenter's point of view and that of the animal might cause the experimenter to fail to understand the animal's behavior. Could this account for the senior author's definition of frustration as "behavior without a goal"?

In order to make a case for frustration theory as distinct from motivation theory we must examine the relevant and specific evidence. It is the purpose of this paper first, to examine the experimental evidence on which the anxiety-reduction theory is based and determine whether frustration theory can adequately account for it. Second, we wish to examine the evidence on which the frustration theory is based and see whether the concepts of anxiety-reduction theory can explain the findings. It is hoped that this process will clarify the differences and permit the design of critical experiments. We believe that

the differences in views do not stem from a disagreement over definitions of motivation or of learning, but rather they hinge on whether or not a qualitative change occurs when an organism undergoes frustration. Thus we believe that the issue to be determined is basic rather than semantic. It seems that if a qualitative distinction exists, it should be recognized and faced so that the benefits of the change in theory can serve their purpose in research design. If a qualitative break does not exist then prevalent theories should be refined so that the apparent discontinuity is explained. When Newcomb (18) distinguishes between threat-oriented and goal-oriented responses, he is recognizing a qualitative factor, but the common term "oriented" tends to obscure the break in continuity and give the appearance of two ends of a continuum.

A casual consideration of the experiments which led to the formulation of the frustration-fixation hypothesis suggests that they merely represent an extension of the frustration-aggression hypothesis and of the frustration-regression hypothesis. Since, however, the explanation of fixated behavior required a qualitative break with learning under motivation, a further condition is implied. This is that frustration not only leads to fixation, but that frustration results in behavior that has no goal orientation. If this is true, then other behaviors associated with frustration should also lack goal orientation. Maier (4), therefore, proceeded to fit the behaviors of aggression and regression into his frustration mechanism and take them out of the realm of motivation. Thus the theory states that when an individual is placed in a problem situation, he first shows problem-solving behavior which is characterized by variability and goal seeking. When, however, the problem is insoluble, when escape is prevented and when pressures force the animal into the problem, he will become frustrated at one stage or another. The appearance of frustration is characterized by a qualitative change in behavior. Problem-solving behavior now is replaced by aggression, regression and fixation and these may appear separately or in various combinations. None of these behaviors is oriented toward the initial goal; in fact they most frequently separate the individual farther from the initial goal. Thus, when frustration occurs, the behavior expressed is an end rather than a means to an end.

The manner in which frustration theory deals with aggression, regression, anxiety, neurosis, and compulsive behavior has been discussed in detail elsewhere (4) and need not concern us here. The question at issue is whether a qualitative distinction must be made. If so, the implications are great. It means that learning theory must exclude frustrated behavior; that social psychology must distinguish between groups organized around frustration and groups organized around common goals (5); that attitudes may be of two types and have two different origins; that therapy must recognize and treat two conditions of the organism; and that all behavior change, training or correction, must be adapted to the condition of the organism.

If fixated behavior can be reduced to motivation, no such implications are involved. It is because the implications of the two approaches are so great that the evidence must be carefully examined and, where the facts are unclear, crucial experiments must be made.

APPLICATION OF FRUSTRATION THEORY TO DATA SUPPORTING THE ANXIETY-REDUCTION HYPOTHESIS

Mowrer (13) is of the belief that organisms can be motivated either by directly felt organic needs or by "the mere

anticipation of such pressures and that those habits tend to be acquired and perpetuated (reinforced) which effect a reduction in either of these two types of motivation" (13, p. 561). He believes that although much of human behavior cannot be prompted by organic drives, it may be explained by "assuming 1. that anxiety, *i.e.*, mere anticipation of actual organic need or injury, may effectively motivate human beings, and 2. that reduction of anxiety may serve powerfully to reinforce behavior that brings about such a state of relief or 'security'" (13, p. 564). Considerable experimental evidence has been collected by various investigators which seems to support the view that fear or anxiety (using the terms synonymously) can be used to motivate the learning or performance of new responses in exactly the same way as hunger, thirst, or sex (10, 12, 14).

Miller (12), using a two-compartment apparatus, one white, with a grid as the floor, and the other black, with a smooth solid floor, tested the hypothesis that fear can be learned and can serve as a drive to motivate behavior. The two compartments in the box were separated by a door which could be opened in several ways: by the experimenter pressing a button, by the rat pressing a bar, or by the rat moving a wheel. Preliminary training showed that the cues in both the white and black compartments were relatively neutral; that is to say, no preference was expressed for either compartment. Then the animals were placed in the white compartment, and given electric shocks. The rats could escape from the compartment by moving the wheel which would open the door between the two compartments. Ten trials were allowed for the learning of the escape response. Shock was then discontinued and pressing the bar instead of moving the wheel now led to escape. On the first non-shock trial the rats exhibited fear responses such as urination, biting, and pawing in the vicinity of the door. On subsequent non-shock trials most of the animals showed gradual elimination of the extraneous movements, and when placed in the box moved the wheel rapidly and went into the black compartment. This test demonstrated that the previously neutral cues in the white compartment had acquired the tendency to motivate trial-and-error behavior and the learning of a new response in exactly the same way as do hunger, thirst, etc. (12). Because of the animal's previous training with the shock and the behavior exhibited, it was postulated that the acquired drive was fear, and the escape from the compartment with its consequent reduction of fear produced a rewarding effect. The results clearly support this postulate.

Because frustration theory does not exclude the learning of avoidance responses, this experiment and others of similar nature (10, 14) are not questioned, except insofar as they may be generalized to interpret all experiments which use punishment as a training device. Since Miller has not attempted to generalize his interpretation to include abnormal fixations, his findings and interpretations are not in conflict with this aspect of the theoretical issue. In other words, we do not reject the possibility of the motivated learning of avoidance and approach reactions. Where we would differ is in the interpretation of results when punishment is used to an extent where frustration is produced in some or in all of the animals. It is for this reason that we differ with Farber (1) who attempts to generalize the concepts of Mowrer and Miller to abnormal fixations.

Farber (1), applying the principle of motivation and learning through anxiety-reduction, believes that behavior fixation can adequately be explained in

these terms. Essentially his hypothesis is that the fixation of non-adaptive responses in shock situations results from uncontrolled secondary reinforcement which is in the form of anxiety-reduction. Since fixation could occur under shock conditions, any diminution in the strength of the fixation which was due to feeding at the locus of shock could be ascribed either to direct interference with the on-going response, or with some process maintaining it.

To test this hypothesis, four groups of rats were given 100 trials in a single unit T-maze with food reward in the goal box on their preferred side. During the last 60 trials, two of the four groups were shocked immediately after the choice-point in the maze during their runs to the food. After the completion of this training, one of the shocked groups and one of the non-shocked groups was fed for two 10-minute periods at the place where the shock was administered to the shocked groups. On the next day, the food was placed in the goal-box on the non-preferred side for all animals and the shock was discontinued. The animals were now tested with the position of the food changed until the original responses extinguished. The number of trials to extinction for all four groups was compared.

The major findings are shown in Table 1. It will be seen from this table that the responses of the shocked rats showed greater resistance to extinction (61.12 trials) than did those of the non-shocked rats (9.71 trials). This finding, in addition to conforming to Farber's hypothesis, is also amenable to explanation in terms of frustration theory. Frustration theory would predict that some rats would be frustrated by the shock, and this frustration would tend to fixate the response. Rats not frustrated by the shock would behave as motivated animals. Further, it is to be seen from Table 1 that the responses of the shocked-non-fed rats showed greater resistance to extinction (61.12 trials) than did those of the shocked-fed group (15.04 trials). Again Farber's hypothesis is supported, but this finding also conforms to frustration theory since it would be predicted that feeding would alleviate the frustration caused by the shock, and hence fewer fixations would result. Although the difference between the non-shock and the shock-fed groups is not significant, the trend of results would favor the position assumed under frustration

TABLE 1*

NO. OF TRIALS TO EXTINCTION FOR ALL S'S IN FOUR SUB-GROUPS

Shocked	Shocked-fed	Non-shocked	Non-shocked-fed
11	17	8	10
14	15	17	6
12	3	7	12
8	7	17	8
57	7	5	10
41	10	8	7
48	4	7	5
19	10	6	7
25	5	9	13
63	26	6	19
14	16	8	6
24	21	10	8
27	4	11	9
33	4	8	4
255†	19	5	7
263†	3	15	2
15	6	6	3
57	33	17	5
33	3	17	9
163	98	11	6
36	10	11	17
193	3	8	28
10	10	7	17
46	27	11	10
Mdn. 33	10	8	8
Mean 61.12	15.04	9.71	9.50
S.D. 74.09	19.24	3.83	5.75

* Table reproduced from Farber (1, p. 122).
† These animals refused to extinguish at all under the ordinary extinction procedure.

theory, since it is very likely that feeding may not remove frustration completely.

The results thus far examined appear to be adequately accounted for under both frustration theory and Farber's anxiety-reduction formulation. However, closer examination of Farber's data reveals certain essential details which have not been accounted for by an anxiety-reduction theory, and which appear to be adequately explained by frustration theory. Thus, frustration theory would expect that some rats would not be frustrated by the shock, and would extinguish as non-shock rats; whereas others would be frustrated and would show high extinction scores. Table 2 presents a frequency distribution of the extinction scores of Farber's animals. It will be noted from this table that 11 rats in the shocked group showed scores which fell within the range (1–30 trials) of the non-shocked-fed group. It is also to be noted that 4 animals in the shocked group have scores clearly falling outside this range, their scores being 163, 193, 255, and 263. Even if we discount the fact that the last two animals still had not extinguished in the trials given we find that the best of these animals deviates 26 S.D. units from the mean score of the non-shocked-fed group. It is obvious that they, at least, form a separate population even if we disregard 9 other animals which exceed the range of the non-shocked-fed group. In a similar manner, we may note that 22 rats in the shocked-fed group extinguished as non-shocked-fed animals, whereas, only two animals in that group exceeded the highest score for the non-shocked-fed rats. The score of the animal which falls within the interval 31–35 may be considered doubtful as to whether it is outside the normal range of scores, but the score which falls within the interval 96–100 is clearly beyond the normal range of scores. If we eliminate the four animals in the shocked group and the one animal in the shocked-fed group which clearly deviate from the normal distribution of scores (non-shocked-fed rats), we find that the mean of the shocked group would be 29.75 instead of 61.12, with a S.D. of 17.5 instead of 74.04; and the mean of the shocked-fed group would be 10.3 instead of 15.04, with a S.D. of 9.2 instead of 19.24. It is obvious that these five animals contributed most of the variance within their respective groups, thereby accounting for the major differences between the shocked-group and the non-shocked-fed group. It is these animals which clearly separate themselves from the rest of the population and whose behavior is clarified by frustration theory. The anxiety-reduction theory does not postulate a break or split in the population, and this may explain why Farber failed to appreciate the unusual nature of his distribution.

TABLE 2

FREQUENCY DISTRIBUTION OF EXTINCTION SCORES OF FARBER'S ANIMALS

Score	Non-shocked-fed	Non-shocked	Shocked-fed	Shocked	
1–5	5	2	8	0	↑
6–10	13	13	7	2	
11–15	2	5	1	5	normal
16–20	3	4	3	1	range
21–25	0	0	1	2	
26–30	1	0	2	1	↓
31–35	0	0	1	2	
36–40	0	0	0	1	
41–45	0	0	0	1	
46–50	0	0	0	2	
51–55	0	0	0	0	
56–60	0	0	0	2	
61–65	0	0	0	1	
66–70	0	0	0	0	
71–75	0	0	0	0	
76–80	0	0	0	0	
81–85	0	0	0	0	
86–90	0	0	0	0	
91–95	0	0	0	0	
96–100	0	0	1	0	
Above 100	0	0	0	4 (low–163)	

Let us now examine the results of two other studies and see whether the data can adequately be interpreted in

terms of frustration theory. Although the authors do not generalize their interpretations to include abnormal fixations, such as we have described, we believe that frustration theory can be used to advantage in understanding the behavior obtained.

Mowrer (15) describes an experiment in which rats were trained to escape from a compartment which had a charged floor. When the floor of the escape compartment was also charged, it was found that the rats persisted in their previous "escape" behavior even though it was now unadaptive. Mowrer believes that such "crazy behavior" necessitates the postulation of two basic learning mechanisms; problem-solving and conditioning. By problem-solving, overt actions or habits are acquired; these reduce drives, solve problems. By conditioning, the emotions or secondary drives are acquired. The so-called "crazy behavior," therefore, is interpreted as follows:

". . . fear of the apparatus is reinforced every time the rat gets shocked therein; and the habit of running in response to this fear is reinforced by the fact that the running, though resulting in a momentary exposure to the shock, leads to a relatively enduring reduction in the secondary drive of fear" (15, p. 593).

It appears that Mowrer has recognized the fact that he is dealing with two distinctly different kinds of behavior, and that a modification in a learning theory which postulates need-reduction is necessary. However, the attempted modification implies some rather tenuous assumptions concerning the operation of secondary rewards. According to frustration theory, one would interpret the original response which removes the animal from the charged compartment as motivated learning. When, however, shock is used to punish the escape response, the animal becomes frustrated, and as a consequence, fixates the response in progress. It is this fixation that supplies the persistent and unadaptive characteristics to the behavior. There is a change, therefore, from a motivated instrumental act to a frustration-instigated act because frustration has been induced. It appears then that the apparently "crazy behavior" which Mowrer describes can adequately be interpreted under the assumptions implied in a frustration theory.

In a similar manner, we may examine another experiment by Mowrer and Viek (17). Using an inhibitory index of fear, these authors compared the number of response inhibitions between two groups of rats placed in an escape situation. Essentially, the procedure employed was as follows: On each trial hungry rats were offered food on a stick for 10 seconds. Regardless of whether the animal ate, shock was applied 10 seconds later. For one group of 10 rats, the correct response to the shock was jumping into the air as a means of turning off the shock (shock-control group). For a second group of 10 rats, shock was applied for the same duration that it took the shock-control animals to turn off the shock, but for this group, no specific response would terminate the shock. This second group will be designated as the shock-uncontrolled group. One trial a day was given to each animal. The authors reasoned that for the first group (shock-control group), "the anticipatory tendency to jump should bring forward with it some of the relaxation which ordinarily follows the termination of shock." Therefore, these animals should be less frightened and should show more tendency to eat than the animals in the shock-uncontrolled group for which this anticipatory response could not be acquired. As seen in Table 3, this prediction is borne out in that only 16 eating inhibitions occurred in 15 days in the shock-control

TABLE 3*

NO. OF RESPONSE INHIBITIONS OBTAINED IN SHOCK-CONTROL GROUP AND SHOCK-UNCONTROLLED GROUP

Successive days	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Total
Shock-control group	0	0	1	0	2	2	2	3	1	1	1	1	1	1	0	16
Shock-uncontrolled group	0	1	4	5	7	6	7	4	7	7	8	8	8	8	7	85

* Cited from O. H. Mowrer and P. Viek (17, p. 196).

group as compared to 85 in the shock-uncontrolled group.

However, it would seem that if fear-reduction is learned then the shock-uncontrolled group should also learn some method for reducing fear. If their situation does not supply an easily learned response, they should merely differ from the first group in that their fear-reducing response would be delayed. Thus the shock-uncontrolled group should show a tendency to reduce its inhibitions in successive days. Comparing the first seven with the last seven days, we find a total of 30 inhibitions for the first seven days and a total of 51 inhibitions for the last seven days, a trend which is just opposite to what should be expected.

Frustration theory would predict that eating inhibitions would be more common in frustrated than in non-frustrated rats. Since the animals in an insoluble problem are the most likely to become frustrated, we would predict more rats showing eating inhibitions in the shock-uncontrolled group than in the shock-control group. This corresponds to the findings of Table 3. Further, we would predict that animals would not be frustrated until they experienced a certain amount of failure, and that all would not become frustrated at the same time. We should expect, therefore, that at the outset, the number of rats showing eating inhibitions would be low and that they would rise as long as the number of frustrated rats increased. Examination of Table 3 shows that for the first two days the animals in the shock-uncontrolled group (the insoluble problem) behaved very much like the shock-control group in that rats showing eating inhibitions were almost entirely absent. Thereafter inhibition became more frequent, and by the fifth day the number of rats showing eating inhibitions in the shock-uncontrolled group reached seven. On one occasion only thereafter did the cases of eating inhibition fall below the number found during one of the first four days. No such trend in the data is found in the shock-control group for which the problem was soluble. Rather it appears that this group finally approached mastery of the problem.

We feel, therefore, that in this instance too, frustration theory can adequately account for the major results and it also can explain additional details in the data.

APPLICATION OF ANXIETY-REDUCTION CONCEPTS TO ABNORMAL FIXATION EXPERIMENTS

Having examined and reinterpreted some of the experimental evidence put forward by the anxiety-reduction theorists, let us examine some of the data on which the abnormal fixation principle is based and thereby test the adequacy of the anxiety-reduction theory.

Using anxiety-reduction and learning concepts, it would be necessary to assume: (1) that a frustrating situation, *i.e.*, forcing the animal to respond to a problem it is incapable of solving, causes anxiety; (2) the animal's aim becomes one of reducing the anxiety, rather than seeking the food reward; and (3) any response that allows relief from the anxiety in the situation will be adopted. Using these assumptions, let us attempt to predict the behavior that results when a rat is placed in an insoluble problem situation.

At first, as would be expected in any trial-and-error learning problem, animals would tend to show considerable variability in behavior. If the problem is insoluble, however, or if it is too difficult, the animal will refuse to jump after several failures. The application of an air-blast or electric shock would then be necessary to evoke a response. It is possible then, that the forcing agent may serve as an unconditioned stimulus for behavior elicited; and the total experimental situation may serve as a conditioned stimulus to which conditioned fear and other responses may become associated. There then might be a shift in the motivational aspects of the situation. That is to say, the response that is made under such conditions would not continue to be directed toward obtaining food, but rather would change to one of escaping from the experimental situation. The response that occurs as a result of the application of the electric shock or air-blast (jumping away from the platform and toward the screen) might be predicted as being the one which will be learned or fixated, because it is the first response which removes the animal from the situation. When the problem is changed so that a solution is possible, it could be predicted that the response developed in the no-solution situation would continue. The basis for this latter prediction could reside in the fact that the animal never tries or is motivated to try the alternative response, since the on-going response is one which satisfactorily reduces the anxiety in the situation. Thus the inability of a rat to learn a discrimination response after having been subjected to an insoluble problem would be ascribed to the absence of a motivating condition which would call forth alternative behavior. Phrasing it another way, it might be said that the animal has no incentive to try an alternative, since the response that is being practiced reduces the conditioned anxiety. In this way, then, an abnormal fixation would be considered as a learned adjustive mechanism to a trying situation, rather than the result of a separate frustration mechanism. This formulation would reduce the abnormal fixation to a very strong habit which is acquired in accordance with current learning principles.

On the surface this hypothesis appears to provide a satisfactory explanation for abnormal fixations, and also tends to fit common-sense impressions and practices. However, there are certain other experimental findings which appear to be inexplicable in terms of an anxiety-reduction theory of fixation.

1. After animals have developed position fixations in the insoluble problem, and are confronted with a soluble discrimination problem, they learn which card punishes and which card rewards (expressed by the manner of jumping), but instead of practicing this learning, they continue to execute the fixated response (7). According to the anxiety-reduction theory, it would seem reasonable to expect that an animal should practice the discrimination which it has learned and which offers more anxiety reduction, rather than continue practicing the fixated response. A response

which gives the animal escape from the stand and no punishment from the card should reduce anxiety more than a response which gives escape and punishment for jumping 50 per cent of the time.

2. It has been found that in replacing one response with another, 100 per cent punishment either causes a very rapid learning of the new response, or causes fixation of the initial response, whereas 50 per cent punishment results in slower learning of the new response for those animals that learn, but the incidence of fixation of the initial response is less frequent (8). No such difference should occur if we assume that the animal is merely attempting to escape from the jumping platform, since the escape from shock factor is constant in the two punishment conditions. If, however, the anxiety-reduction theory included fear of the punishment which occurs as a consequence of jumping incorrectly (striking the locked window, etc.), it would demand the more rapid learning of the new response with 100 per cent punishment for the old response than with 50 per cent punishment, because the anxiety-reduction would be greater with 100 per cent punishment than with 50 per cent punishment. But then the greater number of fixations that are obtained with 100 per cent punishment than with 50 per cent punishment would not be explained.

It is also possible that one might attempt to explain the different effects of 100 versus 50 per cent punishment on the basis of some probability theory. Humphreys (3) found that responses learned under 50 per cent reward were less easily extinguished than responses learned under 100 per cent reward, and he effectively explained his results in terms of expectancies of reinforcement. One might conceivably use a similar approach and explain why 50 per cent punishment is more disrupting to an old response than 100 per cent punishment. However, if this approach were used, only the difference in the number of fixations produced by these two patterns of punishment would be explained, but the fact that 100 per cent punishment is superior to 50 per cent punishment in that it produces more rapid learning of a new response would be unexplained. This dual effect of punishment thus poses a serious difficulty for both a reinforcement and a probability theory.

3. When animals are required to modify the responses developed in an insoluble problem situation, a bimodal distribution of scores obtains; namely, those rats which can solve the new problem and those which cannot (7). If all that is involved is a single motivation mechanism, should not all animals learn an anxiety-reducing response or some other secondary reward and merely differ in degree? The fact that some animals learn a new response at a normal rate and others do not change at all, presents an important problem for any present motivation theory of fixation.

4. It has been observed that a frustrated rat will persist in jumping to its fixated side even when an open window, in which food is visible, is presented on the other side (4). If the animal's behavior is designed to escape from the situation, it would seem reasonable that it should escape via the open window. The apparent compulsiveness of the fixated response is an important problem which is not explained by the new postulates developed by anxiety-reduction theorists.

5. It has also been observed that a rat will fixate a response in progress during a frustrating experience rather than exhibit some trial-and-error behavior which is characteristic of other avoidance learning problems (7). An anxiety-reduction theory, like all other

learning theories, would predict that when an animal is faced with an insoluble problem situation, the behavior at first should be quite variable. The fact that variability is present in some problem situations and not in others is a fact which must be explained if no distinction is made between frustrated and motivated behavior.

6. When a situation is changed so as to require a new response, it was found that guidance causes animals to give up their old responses before they have learned the new response, whereas trial-and-error causes animals to give up an old response only after the new response is learned (9). The fact that guidance is primarily effective for breaking an old response and is not conducive to the learning of a new response, whereas trial-and-error has the opposite effects, is difficult to reconcile with any theory that makes learning a function of relative excitation values. In terms of strength of response a change should occur only when an alternative develops greater strength than a former response.

7. Finally, it has been shown that the behavior of frustrated rats is more rigid than is found in other cases of learning (6, 9). If frustrated behavior is merely a form of learning, based on secondary reward, should not such behavior respond to the same methods of response alteration which are effective for breaking habits based on primary rewards? If a distinction is not to be made, one would seemingly have to postulate that secondary rewards are more basic and form stronger associations than do primary rewards.

The above observations represent the major findings which are basic to frustration theory, and represent the type of evidence which was responsible for its formulation. It is because the findings did not lend themselves to explanations in terms of learning concepts and often went contrary to well-established principles, that a new direction in thinking seemed necessary. Frustration theory thus represents an attempt at a solution to the "neurotic paradox" (15).

SUMMARY AND CONCLUSIONS

We have presented the evidence in favor of both an anxiety-reduction theory and a frustration theory. We have also attempted to explain the results obtained by anxiety-reduction theorists in terms of frustration theory, and have found that the results are entirely consistent with frustration theory. Further, we have found that frustration theory can explain details in the results of the experiments designed to test the anxiety-reduction theory and have found that these details are consistent with frustration theory, but the details have not been explained by the anxiety-reduction theory. Finally, we have attempted to apply the concepts of an anxiety-reduction theory to the experimental findings on which frustration theory is based, and have found them inadequate. Future authors may be able to encompass the problems we have raised, but we believe this will require radical alterations in learning theory. We believe that these problems can be explained more parsimoniously on the basis of frustration theory. This theory does not require the discarding of basic learning concepts, and at the same time permits an extension of behavior analysis by developing a new set of concepts which operate when the organism is in a state of frustration.

In separating motivation and frustration into two separate mechanisms, we have the basis for explaining contradictory behavior trends without having to reconcile them. The implications of making such a distinction are very widespread, and invade practically every field of psychology. If these implications are to be avoided, some adequate

way for accounting for the qualitative discontinuity in present experimental findings must be found. It appears that the crucial experiments must center on the problem of qualitative behavior changes, and it is hoped that the present analysis will point up this need.

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THE AMOUNT OF INFORMATION IN ABSOLUTE JUDGMENTS*

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By absolute judgment we mean that type of judgment in which an *O* of a particular stimulus identifies that stimulus with a single name or number. He does not make a judgment of whether a stimulus is greater than, less than or the same as another. He states that a stimulus has a particular value, whether the value is some physical measure, *e.g.*, 10 millilamberts, or some subjective term, *e.g.*, red. Such judgments are probably made by comparing the present stimulus with some average subjective standard, but the method of obtaining judgments which we describe requires no explicit comparisons. Stevens (8) has called this type of judgment *stimulus rating* and defines the problem as the determination of the accuracy and precision with which physical values can be judged. We prefer not to use that term because it connotes (at least to us) the judgment of magnitudes, and absolute judgments can be made of qualitatively different categories.

With such judgments, whatever they are called, it is meaningful to ask certain types of questions for which the traditional measures of central tendency, dispersion, or correlation do not provide immediate and convenient answers. For example, the stimulus can be thought of as an event which occurs with a certain probability. The distribution of judgmental responses of an *O* can be used to determine how accurately he perceived which of several stimuli occurred at a particular time. In a sense we want to ask how much information an *O* obtained about which of the several alternative stimuli occurred, and for this question it would be convenient to have a measure of amount of information.

It is the purpose of this paper to show that a measure of the amount of information is useful for this type of problem. In addition, we shall show in a later part of the paper that this measure of information gives a direct indication of the minimum number of stimuli from a set which can convey or transmit the maximum information about some other set of events.

Miller and Frick (3) have shown that there are many possible applications of this measure of information and associated concepts in psychology, and they have explained the rationale of the measure. Before going on to our specific problem, however, we shall give a brief description of the measure to aid in clarifying the specific applications outlined in this paper.

A measure of information. Briefly, the amount of information which is contained in or conveyed by the occurrence of an event is a monotonic function of the number of equally likely alternative events which could have occurred. Thus, the amount of information contained in a particular event is not determined by what happened, but rather by what could have happened. For example, when a particular stimulus occurs we get more

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information if the stimulus is one of 20 possibilities than if it is of two possibilities. In this sense, information is equivalent to uncertainty—the more uncertain we are that an event is going to occur, the more information we get when it does occur. (If we know that something is going to happen, we get no information at all when it does happen.) Practically speaking, the terms *information* and *uncertainty* can be used interchangeably, although there are times when it seems more appropriate to use one rather than the other. But in terms of the measurements, a measure of uncertainty is the same as a measure of information.

The particular measure used is the logarithm to the base two of the number of equally likely alternatives, and the resulting unit is called the *bit*. This measure states, in effect, the number of two-choice discriminations which must be made in order to specify one event from a number of alternatives. For example, if an event is one of eight equally likely alternatives ($p = .125$), the amount of information (I) is 3 bits. Mathematically,

$$I = \log_2 \frac{1}{p} = -\log_2 p = 3.0.$$

The three two-choice discriminations are first to specify which of two groups of four, then which of two groups of two, then which of the two in the last group.

If the alternative events in a group do not have equal probabilities of occurrence, then the amount of information in the occurrence of a particular *one* of the events is still computed by taking the logarithm of the reciprocal of the probability of occurrence. If the amount of information conveyed by the long-run average of the entire set is desired, however, then the various amounts of information are averaged, but in the averaging each computed amount is weighted according to its probability of occurrence. Thus,

$$I = \sum p \log_2 \frac{1}{p} = -\sum p \log_2 p.$$

The measure does not require that the alternative events be on a single continuum. Furthermore, if two events are different, there is nothing in the computation which is concerned with how different they are. This is not to say that the measure cannot be applied when a metric difference is known, but rather that the measure can be used whether or not the metric is known. For many types of qualitative judgment, where it would be impossible to obtain a true correlation, the amount of information can be computed and can convey important implications about the data.

CALCULATION OF THE AMOUNT OF INFORMATION FROM ABSOLUTE JUDGMENTS

The type of data matrix we shall be discussing is illustrated in Table I. Various stimulus categories (k, numbered from 1 to S) are presented in a random order to an O who then re-

TABLE I

SYMBOLIC REPRESENTATION OF THE NUMBER OF JOINT OCCURRENCES OF STIMULUS CATEGORY (k) WITH RESPONSE CATEGORY (j)

	1	2	·	k	·	·	S	
1	N_{11}	N_{12}	·	N_{1k}	·	·	N_{1S}	$N_{1.}$
2	N_{21}	N_{22}	·	N_{2k}	·	·	N_{2S}	$N_{2.}$
·	·	·	·	·	·	·	·	·
j	N_{j1}	N_{j2}	·	N_{jk}	·	·	N_{jS}	$N_{j.}$
·	·	·	·	·	·	·	·	·
·	·	·	·	·	·	·	·	·
R	N_{R1}	N_{R2}	·	N_{Rk}	·	·	N_{RS}	$N_{R.}$
	$N_{.1}$	$N_{.2}$	·	$N_{.k}$	·	·	$N_{.S}$	N

N_{jk}: The number of events in cell, jk.
$N_{j.}$ & $N_{.k}$: The row and column totals.
N: The total number of events in the matrix.

sponds by judging the stimulus to be in one of the response categories (j, numbered from 1 to R). Thus we have a matrix with $S \times R$ total cells, and in each cell is indicated the frequency with which a particular stimulus was presented and called a particular response. By adding down columns or across rows, the total number of times that each stimulus was presented ($N_{.k}$), and the total number of times that each response category was used ($N_{j.}$) can be determined.

From these frequencies, the following probabilities can be computed:

$$p(j, k) = \frac{N_{jk}}{N}: \quad \text{Probability of the joint occurrence of stimulus category, } k, \text{ and response category, } j. \tag{1}$$

$$p(j) = \frac{N_{j.}}{N}: \quad \text{Probability of occurrence of } j. \tag{2}$$

$$p(k) = \frac{N_{.k}}{N}: \quad \text{Probability of occurrence of } k. \tag{3}$$

$$p_k(j) = \frac{N_{jk}}{N_{.k}} = \frac{p(j, k)}{p(k)}: \quad \text{Conditional probability of occurrence of } j, \text{ given } k. \tag{4}$$

$$p_j(k) = \frac{N_{jk}}{N_{j.}} = \frac{p(j, k)}{p(j)}: \quad \text{Conditional probability of } k \text{ having occurred, given } j. \tag{5}$$

From this matrix we want to compute the amount of information an O gets about which of several alternative stimuli occurred on a given presentation. This amount of information we can call information transmitted (I_t). Before computing this quantity, however, it is necessary to consider two other quantities which enter into the calculations, and which set limiting values on the amount of information transmitted.

In the first place, the number of alternative stimuli will determine the maximum information transmitted, because an O cannot receive more information than is contained in the stimuli originally. Basically, all we need to know is how much uncertainty there is in knowing which stimulus is going to occur at a particular time. If all stimuli are presented an equal number of times, then the uncertainty is determined simply by the number of possible choices. If the stimuli occur an unequal number of times, then we shall have to use some weighted average measure. This uncertainty in the stimulus we can call the *stimulus information* (I_s), and with the symbols we are using it is defined as

$$I_s = -\sum_{k=1}^{S} p(k) \log_2 p(k). \tag{6}$$

Table II is a simple matrix of hypothetical data which we shall use to illustrate the calculations. With this example four different stimuli are presented an equal number of times. Thus the stimulus information is two bits.

Another factor which will limit the amount of information transmitted is the uncertainty in the responses when the stimulus is not known. For example, if in our example only two responses were ever used, then as far as we can tell the O could obtain only

one bit of information about which stimulus occurred. This uncertainty in the responses we can call *response information* (I_r), and it is defined as

$$I_r = -\sum_{j=1}^{R} p(j) \log_2 p(j). \quad (7)$$

In our example, the response information is

$$I_r = -[.24 \log_2 .24 + 2(.28 \log_2 .28) + .20 \log_2 .20]$$
$$= 1.99.$$

It is less than I_s because all response categories were not used equally often.

The amount of information in the judgmental responses indicates to a certain extent the number of judgment categories which can profitably be used in a rating scale. If, for example, an industrial rating scale with 25 points were used, but the distribution of ratings showed that only three bits of information were obtained from the judgments (indicating that all 25 points were not used equally often), then as much scale efficiency should be possible with only eight points as with the original 25.

Information transmitted. In order to calculate the amount of information transmitted, we can look at the problem from three different points of view. We can deal with the stimulus information as the limiting factor, with the response information as the limiting factor, or we can deal with the entire matrix of cell probabilities. These three ways of looking at the problem result in three different methods of calculation, but since they are mathematically equivalent the technique used may depend only on the form in which data happen to be set up.

TABLE II

TABLE OF JOINT OCCURRENCES OF STIMULUS (k) AND RESPONSE (j) CATEGORIES

		k				
		1	2	3	4	N_j.
	1	76	20			96
	2	24	68	20		112
j	3		12	68	32	112
	4			12	68	80
	$N_{.k}$	100	100	100	100	400

TABLE III

	1	2	3	4	$p(j)$
1	.76*	.20			.24
2	.24	.68	.20		.28
3		.12	.68	.32	.28
4			.12	.68	.20
$p(k)$	.25	.25	.25	.25	

* The cell entries are the $p_k(j)$.

(1) Table III has been set up for what is probably the easiest method of calculation.[1] The logic of this method is as follows: The maximum amount of information which can be transmitted is determined by the response information (I_r). From this quantity, however, a term must be subtracted to account for the fact that each time a stimulus is presented, the same response does not always occur. If a given stimulus does not always produce the same response, then the amount of information transmitted is less than I_r. What we have to subtract is the uncertainty about which response will occur when we know which stimulus has been presented. This uncertainty must be computed for each stimulus separately, and a weighted average determined for all stimuli taken together. We can call this uncertainty *response equivocation* (E_r) and define it as

$$E_r = -\sum_{k=1}^{S} p(k) \sum_{j=1}^{R} p_k(j) \log_2 p_k(j). \quad (8)$$

The information transmitted, which Laemmel (2) has called *intelligence*, is the response information minus the response equivocation. Thus,

$$I_t = I_r - E_r. \quad (9)$$

[1] Newman (4) has published a table of $-p \log_2 p$ for p from 0.001 to 0.999.

If all stimuli occur equally often, then

$$E_r = -\frac{1}{S}\sum_{k=1}^{S}\sum_{j=1}^{R} p_k(j)\log_2 p_k(j). \quad (10)$$

For the example,

$$E_r = -\tfrac{1}{4}[.76\log_2.76 + .24\log_2.24 + \cdots + .32\log_2.32 + .68\log_2.68]$$
$$= 1.03$$

and

$$I_t = 1.99 - 1.03 = 0.96.$$

(2) Table IV has been set up for the second method[2] of calculation. This method is basically the same as the first, except that the stimulus information (I_s) is taken as the limiting value, and from it is subtracted the stimulus equivocation when the response is specified. Thus the proportions shown in the cells in Table IV are the proportion of times that a particular stimulus had been presented where a particular response category was used. The amount of equivocation within each row (E_s), weighted according to the frequency that a particular response is used, is subtracted from the amount of stimulus information (I_s) to obtain the amount of information transmitted. We have

$$E_s = -\sum_{j=1}^{R} p(j)\sum_{k=1}^{S} p_j(k)\log_2 p_j(k) \quad (11)$$
$$= -[.24(.79\log_2.79 + .21\log_2.21) + \cdots + .20(.15\log_2.15 + .85\log_2.85)]$$
$$= 1.04$$

and

$$I_t = I_s - E_s$$
$$= 2.00 - 1.04 = 0.96. \quad (12)$$

These first two methods are identical except that in the first case we deal with probabilities within columns and in the second we deal with probabilities within rows. Superficially, the technique is very similar to that used in an analysis of variance, but no simple mathematical relation between the two types of computation exists.

(3) The third method of calculation is the most basic of the three, and both of the other techniques can be derived from it. Table V has been set up for this method of calculation. With this method, the matrix is treated as a group of individual cells rather than as sets of rows and columns. The basic logic of the method is this: If no information were transmitted to the responses by the stimuli, then the cells would have frequencies determined by chance. The chance frequencies

TABLE IV

	1	2	3	4	$p(j)$
1	.79*	.21			.24
2	.21	.61	.18		.28
3		.11	.61	.28	.28
4			.15	.85	.20
$p(k)$	.25	.25	.25	.25	

* The cell entries are the $p_j(k)$.

TABLE V

	1	2	3	4	$p(j)$
1	.19*	.05	.00	.00	.24
	.06**	.06	.06	.06	
2	.06	.17	.05	.00	.28
	.07	.07	.07	.07	
3	.00	.03	.17	.08	.28
	.07	.07	.07	.07	
4	.00	.00	.03	.17	.20
	.05	.05	.05	.05	
$p(k)$	.25	.25	.25	.25	

* The top entry in each cell is the $p(j,k)$.

** The bottom entry in each cell is the $P(j,k)$, where $P(j,k) = p(j)p(k)$.

[2] This method follows that of Laemmel (2) for computing the amount of transmitted intelligence and that of Shannon and Weaver (7) for describing the rate of actual transmission in communication systems.

in each cell are influenced only by the row and column totals in exactly the same way as for Chi-square. The amount of information computed from these expected cell probabilities ($P(j, k)$) is the maximum uncertainty which can be obtained in the matrix with the given row and column totals; and if this uncertainty were actually obtained, then no information has been transmitted about which stimuli had occurred. The actual uncertainty obtained is computed from the cell probabilities as they occurred. This figure will be less than the maximum because there always is less uncertainty about where frequencies will fall into cells as long as there is any correlation between stimuli and responses. Thus we have a maximum uncertainty, and an obtained uncertainty, and the difference between them is the information transmitted. The extent to which the maximum uncertainty has been decreased by the correlation between stimuli and responses is the extent to which information has been transmitted. We have,[3]

$$I_t = -\sum_{j=1}^{R}\sum_{k=1}^{S} P(j,k)\log_2 P(j,k) + \sum_{j=1}^{R}\sum_{k=1}^{S} p(j,k)\log_2 p(j,k) \quad (13)$$

$$= -[4(.06\log_2.06) + 8(.07\log_2.07) + 4(.05\log_2.05)] + [.19\log_2.19 + .05\log_2.05 + \cdots + .03\log_2.03 + .17\log_2.17]$$

$$= 3.99 - 3.03 = 0.96.$$

INTERPRETATION OF THE MEASURE

In order to interpret the information transmitted (I_t), it is convenient

[3] Equations (9) and (12) can be derived from equation (13) as follows:
First,

$$P(j,k) = p(j)p(k)$$

and substituting in the first part of equation (13), we have

$$-\sum_{j=1}^{R}\sum_{k=1}^{S} p(j)p(k)\log_2 p(j)p(k)$$

$$= -\sum_{j=1}^{R}\sum_{k=1}^{S} p(j)p(k)\log_2 p(j) - \sum_{j=1}^{R}\sum_{k=1}^{S} p(j)p(k)\log_2 p(k)$$

$$= -[\sum_{k=1}^{S} p(k)][\sum_{j=1}^{R} p(j)\log_2 p(j)] - [\sum_{j=1}^{R} p(j)][\sum_{k=1}^{S} p(k)\log_2 p(k)]$$

$$= -\sum_{j=1}^{R} p(j)\log_2 p(j) - \sum_{k=1}^{S} p(k)\log_2 p(k).$$

By equations (6) and (7)

$$= I_r + I_s.$$

Thus the total information or uncertainty in the matrix of expected cell frequencies is equal to the sum of the information in the stimuli and in the responses.

$$p(j,k) = p(k)p_k(j) = p(j)p_j(k)$$

and substituting in the second part of equation (13), we have

$$-\sum_{j=1}^{R}\sum_{k=1}^{S} p(k)p_k(j)\log_2 p(k)p_k(j)$$

$$= -\sum_{j=1}^{R}\sum_{k=1}^{S} p_k(j)p(k)\log_2 p(k) - \sum_{j=1}^{R}\sum_{k=1}^{S} p(k)p_k(j)\log_2 p_k(j)$$

$$= -\sum_{k=1}^{S} p(k)\log_2 p(k) - \sum_{k=1}^{S} p(k)\sum_{j=1}^{R} p_k(j)\log_2 p_k(j)$$

$$= I_s + E_r.$$

Thus the information or uncertainty in the matrix of obtained cell frequencies is equal to the sum of the information in the stimuli plus the response equivocation. In like manner, it can be shown to equal ($I_r + E_s$). Equations (9) and (12) are derived as

$$I_t = I_r + I_s - I_s - E_r = I_r - E_r \quad (9)$$

and

$$I_t = I_r + I_s - I_r - E_s = I_s - E_s. \quad (12)$$

to think of the stimulus as being a symbolic representation of a series of possible events, as illustrated in Fig. 1. Here we have a series of possible events (which need not be on a true continuum), and we are representing ranges or classes of events with a single stimulus value. For example, all events between E_0 and E_1 are represented as the stimulus value S_1. This S_1 is then responded to as R_1, which may be anything from a numerical report to a course of action. (It should be remembered that a stimulus may represent a class of events which are stimuli of the same kind.) We have complete freedom in deciding which stimulus value will represent which class of events, and there need not be a one-to-one correspondence between the events and the stimulus representation. In fact, as we shall point out later, there usually should not be a one-to-one correspondence. But we shall nearly always want to have a one-to-one correspondence between the stimulus and the response.

When we talk about the amount of information transmitted, we refer to the amount of information about the event continuum which a particular range of stimulus values can transmit to an O. It does not indicate how much information is in the event continuum, or how much could have been transmitted with some other type of symbolic representation or with some other range of stimuli. Two examples will serve to illustrate the difference between this situation and other situations which are superficially similar.

Suppose that colors are used to represent positions on an airport surface, and we do an experiment in which 20 different colors are judged by color naming. From the data matrix we compute that three bits of information were transmitted by the colors. This

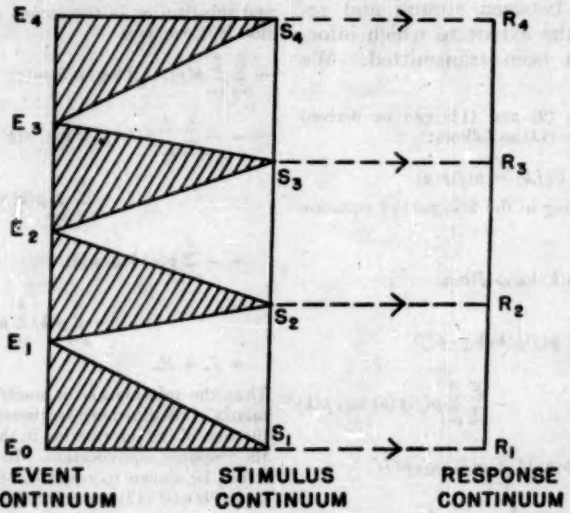

FIG. 1. The relation between the event, the stimulus and the response continua. A group of events is represented by a single stimulus, and in the ideal case a one-to-one relation between the presented stimulus and the verbal response or action holds. Which events (or how many) are represented by which particular discrete stimulus is an arbitrary matter.

figure means that just as much information about positions on the surface of the airport can be transmitted by the use of eight colors as by the use of the original 20. Thus, there is no inefficiency in the use of the smaller number of colors.

There are two factors which make this efficiency with the smaller number possible. The first is that we can select any discrete values of stimuli; we are not simply pooling stimulus values. The second factor is that an exact relationship between surface positions and the color used to represent the position can be maintained. In other words, there is no error in the assignment of the color code to the positions on the airport surface. If such error did exist, then the use of fewer stimulus categories would result in less information transmitted.

The numerical example we have been using will illustrate this point. We started with four stimuli and four responses, but computed that approximately one bit of information was transmitted. With the interpretation we have given, we should be able to transmit just as much information with only two stimuli and two responses. Reference to Table II shows that if only the first and fourth stimuli were used, with one response category for each, we would have no errors for the 2×2 matrix. This situation would give one bit of information transmitted. All the events previously represented by the second stimulus would now be represented by the first stimulus, and there would be no loss of information. Although the coding was coarser, fewer errors would be made, and the information transmitted would remain the same.

It is clear that our real criterion is the correspondence between the event continuum and the response continuum. We are asking how much of the information in the event continuum is represented by the response continuum. There are other ways of measuring the correspondence between the two when the event continuum is quantitative. A product-moment correlation coefficient, or a total error variance analysis could indicate the degree of relationship between these two scales. It may not be immediately obvious from the relations we have stated that in fact the precision with which the responses represent the event continuum will not become poorer if fewer categories are used, but experimental evidence for this fact will be presented by the authors elsewhere.

As a second illustration, suppose that ratings on a scale of efficiency are obtained from judges about a group of eight men. The stimulus-response matrix looks as it did before, and we compute that two bits of information have been transmitted. Now, however, we cannot say that we could have obtained just as much information about the relative efficiency of the eight men if we had just used four men. We want information about all of the men, and we have no means of pooling the men into groups to be represented by just one man with no error in the pooling. Also, we cannot use a rating scale of four steps, because it would be too coarse to allow us to rate some of the men accurately. The main point of distinction is that the information transmitted does not tell us that data can be pooled. The events which are being represented by the stimuli can be pooled (if done accurately) and each group represented by a discrete stimulus. But if stimuli and responses are simply pooled into groups, information will be lost as long as there is some correlation between stimuli and responses within the groups. Again, in Table II, if we pooled stimuli and responses

into two sets each, there would still be some errors, and the amount of information transmitted would be somewhat less than one bit.

THE CONTINGENCY COEFFICIENT

There is one statistical measure commonly used by psychologists which has many of the same properties as the measure of information. It is the contingency coefficient (C). Normally it has been used and interpreted as a correlational measure for non-metric data, and its use for this purpose perhaps has prevented some of its other uses becoming apparent.

If the proportions in Table V were changed to frequencies, then we would have a table from which Chi-square and C could be computed. We have computed C^2 for the data in our example, and it is 0.56. This coefficient can be interpreted in much the same way as the amount of information. For a matrix of a given size (number of rows and columns) there is a maximum C^2 which can be obtained. This maximum is $(k-1)/k$, where the number of rows (k) is the same as the number of columns. (An exact limit for cases in which the number of rows and columns is not equal has not been determined.) Thus for a 2×2 matrix, the maximum C^2 is 0.50. If a C^2 of approximately 0.50 is obtained from a matrix larger than 2×2, then the matrix can be decreased by selecting certain stimulus categories, and by grouping corresponding response categories, and the correlation will not appreciably change. Thus if C^2 is known, the minimum number of rows and columns which would allow that value to be obtained can be determined. This number is the number of stimulus categories for which practically perfect response correspondence can be obtained. Thus C can be used to obtain an answer to the type of question with which we are concerned.

The fact that there is a limiting value of C has frequently been considered a disadvantage of the measure. Actually, the implications of this limit are the same as the implications of the amount of stimulus information in limiting the amount of information which can be transmitted. In a 2×2 matrix, the maximum information transmitted is one bit, and the maximum C^2 is 0.50. If only the two diagonal cells are filled in the matrix, these maximum values will be obtained. The true interpretation is not that this is the maximum correlation which could be obtained, or the maximum amount of information which can be transmitted. Rather, the interpretation should be that with these few categories it is impossible to demonstrate a greater C, or a greater capacity for information transmission.

Although both the amount of information and C are computed from the same type of data matrix, and although the beginning steps are the same, the two are not mathematically equivalent. If the distribution of errors of judgment is known, then an equivalence between the two measures can be worked out. The two measures respond to sampling errors in different ways, however, so that for finite and reasonably small samples one measure cannot be derived directly from the other. On the other hand, with a Gaussian distribution of errors, for every amount of information computed, a C can be estimated. This estimation can be used to determine approximate probabilities for rejection of the null hypothesis.

The amount of information seems to us to be a better measure for several reasons. First, it is considerably easier to calculate. Second, it is more directly interpretable in terms of number of categories. Third, the limiting

value of C approaches an asymptote of 1.0 as the size of the matrix is increased, while amount of information can become indefinitely large. Finally, C is too apt to be interpreted as a correlation measure. It does not suggest new ways to attack problems, or new interpretations of data as easily as the information theory context does.

Another application. The similarity between these two measures, however, suggests another application of the measure of amount of information. It is as a preliminary determination of the number of categories necessary to get a correlation as large as the data can produce. For example, if in a preliminary test of the relation between magnitude of GSR and light intensity we get a C^2 of 0.50, or two bits of information in the matrix when more than two intensities are used, then we know that we can demonstrate the maximum correlation by using only two light intensities (the maximum and minimum) and by lumping all GSR magnitudes into two categories. The correlation will not be decreased by doing this, and the amount of information about the relationship will not decrease. If for practical reasons it is desirable to use fewer categories, and perhaps more cases in each category, then we know that we shall not have lost any information in so doing.

SELECTION OF THE STIMULI

Computation of the amount of information transmitted indicates the minimum number of stimulus categories which can be used to obtain the maximum amount of information about a given set of possible events. The computation, however, does not indicate which particular stimuli should be used. There are two major criteria in selecting the particular stimuli: The first is to have stimuli which *O*s will tend to categorize by using each response alternative as often as each corresponding stimulus is presented. If relatively few categories are used, there is probably little difficulty in maintaining this criterion by instruction. The second criterion is to have as few errors as possible.

If the stimuli are on a true continuum, such as brightness or loudness, then we want to select stimuli which are equally discriminable from each other. If we assume that errors of judgment of loudness, for example, are distributed according to the Gaussian curve of errors, then the fewest total errors will occur if the stimuli are selected to be an equal number of standard scores apart. For example, suppose we have two stimuli locating the end-points of our scale, and we want to place a third stimulus in between in such a manner as to minimize the judgmental errors. If we place the third stimulus halfway between the other two on a scale of discriminability, then errors will be minimized. If the third stimulus is moved closer to one of the other two, errors of confusion will increase between those two at a greater rate than they decrease between the middle stimulus and the other end-point. With other types of error distribution, of course, this rule might not hold.

We can select stimuli according to these criteria by inspection of the data and perhaps a little trial-and-error. It would be better, however, if we could determine the psychological properties of the stimulus continuum in such a manner that we could make a more exact prediction of the stimuli which should be selected, and for many different circumstances. Basically we need a scale of equal discriminability. A scale of integrated difference limens could be the answer,

but difference limens are obtained from quite a different judgmental situation. If we could assume that judgments of the type we have been talking about are made as equal-interval judgments, and that the criterion for equal intervals is discriminability, then we could use the scale computed directly from the responses as our scale of equal discriminability. Two previous authors, however, have shown that absolute judgments are not made with all response categories used as equal discriminability steps. Usually judges discriminate between the end categories more than between the middle categories. Saffir (6) first pointed out a method for correcting the scale and establishing a true scale of equal discriminability. He called his technique the method of successive intervals. Later Attneave (1) developed a technique which he called the method of graded dichotomies.

CONSTRUCTION OF A SCALE OF EQUAL DISCRIMINABILITY

This section describes a third technique for constructing a scale of equal discriminability. The scale values obtained are basically the same for all three techniques, although the different techniques react differently to errors of sampling.

The computations will be illustrated with data obtained in an experiment which required absolute judgments of the loudness of tones presented once every six seconds. There were twenty stimulus intensities ranging from 5 to 100 db S.P.L., in 5 db steps. Judgments were required on a 21-point scale (0–20), where "0" meant that a tone was not heard, and "20" meant it was the loudest in the series. The computational steps are as follows:

(1) *Arrange the data in a matrix of the form illustrated in Tables I and II.*

(2) *Cumulate the frequency of responses by successive response categories for each stimulus separately.* For example, the stimulus intensity of 60 db produced judgments at response categories of 3, 4, 5, 6, and 7 with frequencies of 32, 35, 15, 11, and 7. The cumulated frequencies are 32, 67, 82, 93, and 100. Thus the number 93 means that this particular stimulus was called "6" or some lower number 93 times.

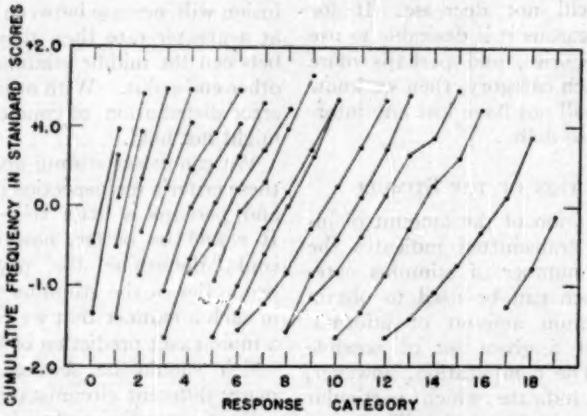

FIG. 2. Cumulative frequency of loudness judgments expressed in standard scores. Each curve is for a different stimulus intensity, starting with the lowest intensity on the left. Each point represents the proportion of times that a particular stimulus has been called a particular response category or lower. Data from one *O*, 100 judgments for each stimulus intensity.

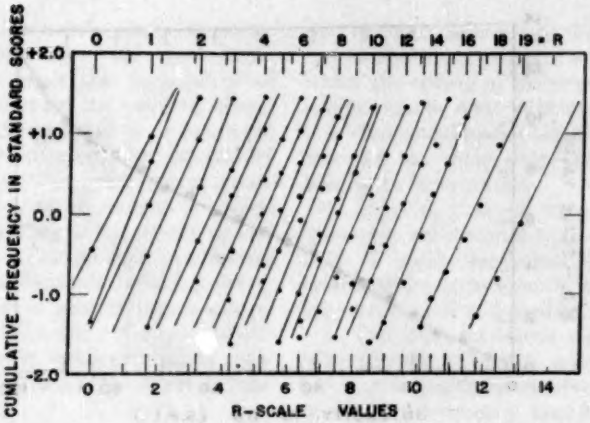

FIG. 3. Cumulative frequency of loudness judgments expressed in standard scores after the spacings of the response categories have been adjusted for equal discriminability. The data are the same as in Fig. 2, but have been plotted with the adjusted scale of response categories. The actual response category is shown at the top. The *R*-scale value for the categories is shown at the bottom.

(3) *Convert the cumulative frequencies into proportions of times that each stimulus is called a particular response category or lower.* In the example given, since the total presentations of each stimulus were 100, the proportions are: .32, .67, .82, .93, and 1.00.

(4) *Convert the cumulative proportions into their equivalent standard scores.* In the example these become −.47, +.43, +.92, and +1.48. No standard score can be determined for a proportion of 1.00, of course.

These cumulative frequencies expressed in standard scores for all 20 stimuli are plotted in Fig. 2. It is apparent from this plot that all response categories were not used with equal discrimination, since the curves for the lower intensities are much steeper than those for the higher intensities. What is needed is some means of adjusting the positions of the response categories so that all curves will have the same slope. If the errors are normally distributed, then all curves will also be straight lines. The technique used is to determine the change in the ordinate for a unit change in the abscissa, and to use this measure of the slope to determine the relative spacings for the response categories.

(5) *Compute the average difference in standard scores for each successive pair of response categories.* For the particular stimulus we have been using as an illustration, the difference between response categories 3 and 4 is .90, and that between 4 and 5 is .49. However, the curves for four other stimulus intensities also can be used to compute the difference for categories 3 and 4, and three others for the difference between 4 and 5. For each successive pair of categories the average of all differences which can be computed is determined. The number that can be averaged will vary depending on which pair of response categories is being considered.

(6) *Arbitrarily assign the lowest response category a value of 0, and then add the successive differences to obtain the values for each successive category.* A new scale (the *R*-scale) value has now been determined for each category, and a plot of the cumulative curves with these new scale values is shown in Fig. 3. Now all curves are essentially straight lines with the same slope.

(7) *Determine the R-scale value equivalent to the median response for each stimulus intensity.* It is easiest to do the calculations from the cumulative curves

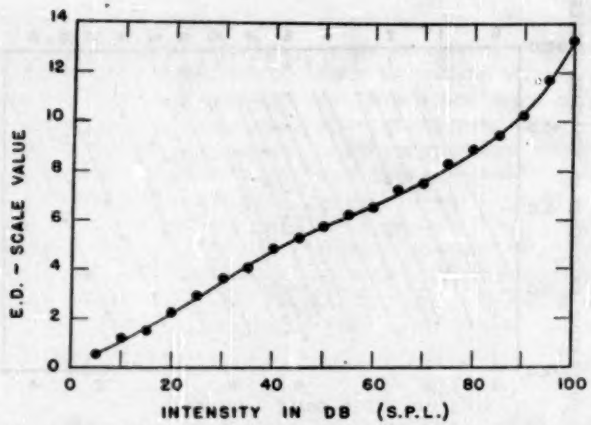

FIG. 4. The equal-discriminability scale values as a function of stimulus intensity for the data shown in Fig. 2. The *E.D.*-scale value for each intensity is determined from Fig. 3. It is the *R*-scale value which (for each stimulus intensity) corresponds to a standard score of 0 on the ordinate. In other words, it is the *R*-scale value equivalent to the median response for a particular stimulus intensity.

as shown in Fig. 3, and any of a number of techniques can be used: Visual estimation, linear interpolation, method of averages, least squares, etc. Now a scale value has been determined for each stimulus intensity. The final scale computed with this technique is shown in Fig. 4. We have called it an equal-discriminability (*E.D.*) scale.

It should be pointed out that the final scale is one in which equal distances on the *E.D.*-scale represent equal extents of discriminability or confusability. The correction of the response scale has resulted in a scale in which equal distances on the *E.D.*-scale represent equal tendencies to call two stimuli the same response. Thus any peculiar use of the response categories has been ruled out. For example, occasionally *O*s refuse to use particular categories. The final scale computed has cancelled out this effect by being only a measure of the extent to which two stimuli are called the same response, regardless of what that response is. Furthermore, neither the *R*-scale nor the *E.D.*-scale has an absolute zero. Thus the scales have only the properties of equal-interval scales, not of ratio scales.

If we now wanted to select just four stimuli from this range of intensities, we would select 5, 40, 82, and 100 db, since these four stimuli are equally spaced on the *E.D.*-scale.

Practical considerations. Frequently practical considerations require a number of stimuli greater than the minimum number necessary to get maximum information transfer. In such cases the proper selection of the stimuli is even more important if information transfer is to be maximized. There is one particular situation in which proper selection of the stimuli by using the *E.D.*-scale can partially offset some disadvantages produced in the original event continuum.

Suppose that practical considerations require us to use a number of categories sufficiently large that we shall always have errors of judgment. Suppose also that the events which are being represented do not occur equally often. It is apparent from the calculation formulae that an unequal probability of occurrence will decrease information transfer. However, this effect can be partially offset by spacing stimuli far apart when they

are used frequently, but closer together when they are used infrequently.[4] Thus the probability of error is decreased for frequent occurrences at the expense of an increased probability of error for infrequent occurrences. For every set of event probabilities, there should be some optimum spacing of the stimuli on the *E.D.*-scale. The precise determination of the optimum spacing for a particular set of probabilities is a complicated calculation. But it should be possible to work out some relatively simple rules-of-thumb to handle these cases.

Ideally, of course, we would prefer to avoid this type of problem by grouping the various events into sets which have equal probabilities of occurrence. Thus all stimuli would occur equally often, and the system would produce maximum information transmission. As a practical matter, however, other considerations will frequently determine the groupings. The next best thing is to compensate for the unequal probabilities as we have suggested.

SUMMARY

1. With absolute judgments, or stimulus ratings of stimulus categories, the judgments of an *O* indicate how accurately the *O* perceived which of several alternative stimuli occurred on a particular presentation, or how much information the *O* obtained about which stimulus occurred.

2. The amount of information which the stimuli transmit to an *O* can be measured in bits.

3. This measure gives an estimate of the minimum number of stimulus categories which can be used to transmit the maximum amount of information in those cases where the stimulus is a symbol for a group of events, and where the coding of the events to the symbol can be done with no error.

4. The contingency coefficient has some of the same properties as the amount of information.

5. In order to select the stimuli for maximum information transmission a scale of equal discriminability is required, and a technique for constructing such a scale is described.

6. The scale of equal discriminability can also be used to select stimuli which will partially compensate for an unequal frequency distribution of the events being represented.

[4] This is essentially the problem of coding against noise in communication systems by the adjustment of relative input rates. This problem is discussed by Laemmel (2) and Ruark (5).

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PERCEIVED SIZE AND DISTANCE IN VISUAL SPACE

BY ALBERTA S. GILINSKY

In a recent survey of the status of research on visual perception, Graham (8) points out that the facts of space discrimination are still poorly understood. "In particular a long series of researches has not resulted in those systematically determined functional relations which, as Holway and Boring (1941) say, are 'wanted and wanting'" (8, p. 876).

The purpose of this study is to develop a quantitative formulation of visual space perception expressing such functional relations. Functions relating visually perceived size and distance to true size and distance are rationally derived and then applied to the results of a variety of visual size and distance experiments.

The theory is expressed quantitatively in two simple, interrelated formulas—for perceived distance and perceived size, respectively. These two formulas are derived mathematically in three different ways: First, the two formulas are rigorously derived from the basic metric of visual space as established mathematically (for binocular vision) by Luneburg (12). Second, the same two formulas are mathematically derived (somewhat less rigorously but without restriction to binocular vision) from the known principles of visual perspective. Finally, the same two formulas are derived by a simple inductive method of mathematical composition for the two boundary laws of size constancy and retinal image (visual angle). All three methods of derivation yield the identical pair of formulas to express a unifying law of visual space perception.

In place of three different observer constants and several unstated scale constants contained or required to be supplied in Luneburg's formulas, the present simplified formulas contain essentially a single constant or parameter, A. This parameter, A, varies for different observers and with the conditions of the experiment. The numerical value of A for a given observer may be expected to depend upon the availability of cues to distance. Through the perfection of techniques whereby the value of A may be reliably determined for individual observers and for different conditions, the two formulas can be made into a useful tool to enable individuals to estimate true sizes and distances from visual observations at a distance. A practical measure or specification may also thus be made available for classifying or grading individuals with respect to their distance judgment or spatial discrimination.

The general validity of the two formulas is supported by visual size and distance experiments planned and conducted for this study. They are also checked by visual size experiments by prior investigators (9, 16) and other observations recorded in the literature, such as perceived size in stereoscopic photographs (18), the alley experiments (2), and the visual speed of moving objects (5). A further striking check (seemingly unpredicted) is the applicability of the same distance-formula to perceived distance determined by *auditory* stimulation (17). This variety of evidence indicates that the derived relations and the unifying law which they express may have considerable generality.

Visual Space. We open our eyes

and perceive a three-dimensional world in which objects have perceived size, form, and localization. Our experience is called *visual space.*

Visual space and physical space are not identical. Neither is visual space in three dimensions a *proportional* replica of objective space in three dimensions. One is a distorted transformation of the other. As Ogle has stated:

". . . an object in objective space may be displaced without deformation and without changes between related points, . . . in visual space, this is not true. As an object recedes, not only does it appear to become smaller but its shape appears to change, this change being greater in the depth extent than in height or width. Many other discrepancies between the two spaces have been established. . . . It is only important that, whatever relationship exists between objective and visual space, it should be fairly stable if the individual is to act effectively in the physical world" (13, p. 11).

In the prior search for a simple law of visual space perception, two principal theories have been held—each incompatible with the other, and each beyond certain limits becoming inconsistent with known phenomena and relations.

1. At comparatively short distances, well within the range of accustomed experience or confirmation by other senses, the organism tends to utilize corrective cues so as to compensate for the distance and the corresponding diminution of retinal size. Observations under such limiting conditions have given rise to the so-called law of size constancy, whereby the perceived size of an object is taken as constant and independent of retinal size (1, 8, 9).

2. At the other extreme, if size of retinal image is assumed to be the sole cue to perceived size, and if all depth or distance perception is ignored, the so-called law of the retinal image or visual angle is obtained, whereby the perceived size of an object is taken as simply proportional to the size of the retinal image (or visual angle) and therefore inversely proportional to the distance of the object. This theory is more nearly true at very great or astronomical distances (1, 3, 8, 9).

Neither the law of the retinal image nor the opposite theory, the law of size constancy, can be sustained over the entire range of distances and conditions (2, 4, 8, 9). As Boring (2, 4) has shown, each view describes only a special limiting case. An adequate general theory must be capable of satisfying these boundary conditions and of providing, as well, for a smooth continuous transition over the large intervening range between the extreme boundary laws. The aim is to find a unifying law applicable to all distances and to all conditions of visual perception.

The various theories of perceived size may also be compared by relating them to another concept—"the projection theory of visual perception" (10, 12). If the two base points are the centers of rotation of the eyes and the projection lines are the optical axes, the intersection of the two projection lines will be at the actual point fixated and therefore at the true physical distance D. If the retinal image is projected to this true physical distance D, the perceived size will be undiminished by distance and the law of size constancy is obtained. If, on the other hand, the retinal image is always projected to an arbitrary constant distance, independent of true distance, the perceived size will be simply proportional to the size of the retinal image, without regard to distance, and the law of the retinal image is obtained. As stated above, neither of these two extreme laws is adequate.

The perception of distance or depth is a fact, and must be included in a generalized theory. But that does not mean the true or physical distance D, for *physical* distance is not perceived. What is sensed or perceived is the *perceived distance* d, and that is the only distance that is subjectively realized and therefore the only distance at which the projection can, logically and consistently, be made. If the size (in radians) of the retinal image or visual angle is φ (phi), and if the perceived distance is d, then the *perceived size* s, is given by the simple relation

$$s = \varphi \cdot d. \qquad (1)$$

According to our hypothesis, this is the basic, unifying relation between perceived size and perceived distance.

The significance of this concept for our understanding of space perception is that it unifies a large number of previous observations, embracing both size constancy and retinal image as limiting cases, and bridging the gap between these two extremes.

Perceived distances are foreshortened. The perceived distance d increases with the true distance D but at a reduced and diminishing rate. Consequently the law of perceived size given by equation (1) is intermediate between the law of retinal image and the law of size constancy.

Moreover, the perceived distance d approaches a finite limit A when the true distance D increases to infinity.

As Luneburg has stated: "Astronomical objects like the sun or the moon are seen at finite distances; their sensed size is also finite and in no way proportional to astronomical dimensions. Even the sky itself gives the impression of a dome of finite radius" (12, p. 1).

As thus pointed out by Luneburg, and earlier, by James (10), there exists a maximum limit of perceived distance. Whereas physical space is infinite, visual space is finite. This concept of an upper limiting threshold (or limen) for spatial discrimination or distance perception is consistent with the known facts of spatial discrimination and with sensory discrimination in general (8, 19). The question as to what physiological mechanism or receptor process governs or underlies this maximum limit of visual distance is immaterial for our purpose.

Luneburg was principally concerned with investigating the geometrical character (hyperbolic, Euclidian, or elliptic) of the tridimensional manifold comprising visual space. He shows that "the geometry in any manifold can be derived from its metric, i.e., from a rule for measuring small line elements." The problem is thus "to establish a metric for the manifold of visual sensations" (12, p. 2). The basic metric given by Luneburg establishes the general relation between the two spaces, visual space and physical space. Using Luneburg's basic metric as our point of departure, we shall derive our own psychometric coordination, based on the Euclidian geometry, for formulating the relation between visual and physical space.

DEFINITIONS: PERCEIVED SIZE AND DISTANCE

The real *objective* size of a physical object is subjectively unknowable. We can, however, apply the familiar physical and geometrical operations of measurement, starting with the simple relation of equality, so as to determine the *relative* physical sizes of objects. A standard unit measuring rod supplies the objective scale. The objective size measured on this scale will be designated by S_0.

Similarly, the *subjective* size s of visual objects cannot be objectively determined. We can, however, apply various procedures of matching and comparison, limited to an individual observer and constituting psychophysical measurement. Starting with the simple relation of equality, we may thus determine (for a given observer) the *relative* perceived sizes of different objects, and also the relative perceived sizes of the same object at different distances. Moreover, we can select one of these viewing distances as "the normal viewing distance" δ (delta) and we can call the corresponding perceived size of the object the "true size" S to be used as a standard reference size for the visual perception of the object. This concept has been anticipated by William James:

> *"Out of all the visual magnitudes of each known object we have selected one as the* REAL *one to think of, and degraded all the others to serve as its signs.* This 'real' magnitude is determined by aesthetic and practical interests. It is that which we get when the object is the distance most propitious for exact visual discrimination of its details. This is the distance at which we hold anything we are examining. Farther than this we see it too small, nearer too large. And the larger and the smaller feeling vanish in the act of suggesting this one, their more important *meaning*" (10, p. 179).

The physical measuring rod used for objective size becomes also the measuring rod for subjective size provided the rod is held at the normal viewing distance δ (delta). We assume equality of *visual* size when object and measuring rod have equal physical size and are held at identical viewing distance.

When the object is more remote, we hold the measuring rod at the normal viewing distance δ and then transfer its visual size to superposition upon the object at its perceived distance d. The equivalent procedure is to transfer the visual size of the distant object to the measuring rod (or comparison standard) held at the normal viewing distance δ. This technique must be included in the instructions to the observer and must be learned by practice. In each case a recalled visual size must be compared with an observed visual size. The two visual sizes (object and standard at disparate distances) must not be compared directly by simultaneous viewing because that tends to introduce comparison of retinal images instead of comparison of subjective sizes.

Finally, we have to define and establish a measure of *subjective distance*. For this purpose we note that the transformation from physical to visual space is cubic or isometric at short distances such as the normal viewing distance δ and that the depth dimension becomes perceptively compressed at greater distances. Accordingly, the same measuring stick used for perceived size may also be used as the measuring stick for perceived distance; in each case it should be held at the normal viewing distance δ.

A physical cube will be perceived as a cube if viewed at the normal viewing distance δ. At more remote distances the perceived thickness or depth will diminish more rapidly than the frontal dimensions. The receding solid will cease to appear as a cube in visual space unless it is physically elongated as it recedes to increasing distance from the observer. The sun and the moon lose their spherical thickness and appear as flattened disks against the sky.

A line in physical space may be bisected. A line in visual space may be bisected. The two division points are not necessarily identical. If the line

recedes from the eye, as a distance, the two divisions will be different.

A yardstick in physical space may be moved to different points or positions without changing its *physical* length, but its visual length will change. A yardstick in visual space may be moved to different points or positions without changing its *visual* length, but its physical length would have to be changed. The constant physical yardstick is used for measuring physical lengths. The constant visual yardstick is used for measuring visual lengths (perceived sizes and distances).

In order to correlate the two yardsticks, we define the *visual* yardstick as the perceived size of the *physical* yardstick held at a convenient normal viewing distance δ (delta) from the eye. By this expedient we get around the difficulty of correlating two incommensurables—an objective magnitude and a subjective magnitude.

To apply the visual yardstick to a distant object, it is assumed that the visual yardstick is moved to the distant object without changing the *visual* length of the yardstick.

A distance from the observer (O) is measured physically by repeatedly applying the physical yardstick, thereby dividing the distance into *physically* equal divisions. The successive divisions will appear shorter as they recede from the observer. To measure the distance *visually*, the visual yardstick must be applied repeatedly so as to divide the distance into *equal visual divisions*. These successive divisions will be longer as they recede from the observer.

The meaning of perceived size s is thus linked to the meaning of perceived distance d. Visual spatial extents are measured in all three dimensions of visual space by one and the same subjective measuring rod or visual yardstick. (The physical length of this yardstick will vary with distance and direction. Only at the standard viewing distance δ are the physical and visual yardsticks equal and invariant.)

For the purpose of the present study, it is extremely important to draw a clear distinction between perceived distance and estimated distance, corresponding to the similarly important distinction drawn by Hering and others (2) between perceived size and estimated size. Perceived distance is phenomenal or apparent distance, comprising exclusively the direct product of stimulation (visual and muscular). Estimated distance includes an intellectual correction of perceived distance, derived from past experience and training, to arrive at a more informed inference or judgment of true distance. Perceived distance is represented by the reduced magnitude d. Estimated distance is an attempt, conscious or unconscious, to estimate (from d) the true distance D.

When a subject says that a distant object appears to be one mile away, he means that it appears as far away as an object known to be one mile away. That is an absolute judgment (based on past experience) and not a measure of perceived distance. For a consistent definition of *perceived distance* d, when we say that a perceived distance is 40 yards we mean that the distance is perceived (in a subjective scale of visual distance) as twice as big as a perceived distance of 20 yards, or 40 times as big as a perceived distance of one yard—40 *visual* yardsticks laid end to end. The physical lengths of these visual yardsticks would increase progressively as they recede from the eye. Accordingly, the perceived distance of an object 100 yards away may be

only 30 or 40 yards, although we may have learned, by training or experience, to judge it as 100 yards away. Similarly, the perceived distance of the horizon or the moon may be only 50 yards away by this definition.

For a consistent definition of *perceived size s*, the constant visual yardstick must be applied (in imagination or projection) to the distant object at its *perceived* distance. *O* should be guided to concentrate on the apparent size of the distant object at the distance at which *O* projects or visualizes the distant object.

Imagine an arm, capable of indefinite extension, holding the visual yardstick, maintaining the apparent length of the stick unchanged as it moves through space to make possible a subjective superposition of the measuring stick upon the distant object, say, the moon. If the imaginary arm carries the measuring stick farther than the perceived distance of the moon, the resulting measurement of perceived size will be too big. If the distance to which the visual yardstick is carried falls short of the perceived distance of the moon, the resulting measurement of perceived size will be too small (cf. 11, p. 237). The various answers given by different individuals to the question, How large is the moon?—answers which vary from ¼ inch to 30 feet—illustrate this strikingly. Instructions to concentrate on the apparent size of the moon at its perceived distance yield equivalence matchings within a much narrower range, 4 to 20 inches. The variation which remains must be largely attributed to the fact that individuals differ in their perception of spatial depth or distance, particularly when, as in this case, the actual distance is so great and beyond the range of experience through the senses.

PERCEPTION OF DEPTH

In the perception of visual space the two essential elements are size of *retinal image* (or visual angle) φ (phi) and the perception of depth or distance *d*. Both retinal and muscular sensations are involved.

In binocular vision the basic essential for perception of depth or distance is the *convergence* (or the horizontal disparity or parallax) γ (gamma). The interocular or interpupillary distance provides the psychophysical base line for the geometry of binocular space perception. Although Luneburg refers to γ sometimes as "horizontal disparity" and sometimes as "bipolar parallax," he writes:

> "The meaning of γ is clear: the angle subtended by the lines of sight at the point of *convergence, P*" (12, p. 13).

In monocular vision the corresponding physiological cue is the muscular or kinaesthetic sensation of *accommodation*.

In addition to these primary or basic cues of convergence and accommodation, there is a variety of supplementary clues which aid the perception of spatial depth. These associated or auxiliary clues include linear perspective, aerial perspective, interposition, movement parallax, texture gradient, light and shade, and apperceptions from past experience.

Luneburg does not attempt a physiological explanation of the relation of size perception to convergence, but he does consider this relation to be a necessary hypothesis. "Our scale of size seems to contract with increasing values of γ" (12, p. 103).

We need not consider the specific physiological mechanism whereby depth or distance in visual space is perceived. Such discussion is not essential for the development of the main thesis of the present study.

THE BASIC FORMULAS

The mathematical derivations of the two fundamental interrelated

formulas for perceived distance and perceived size are given in the Appendices. All three methods of derivation yield the identical pair of basic formulas.

The basic formula for *perceived distance* is:

$$\frac{d}{D} = \frac{A}{A + D}, \qquad (I)$$

where d = perceived distance, D = true (physical) distance, and A = maximum limit of perceived distance for a given O under given conditions.

In fact, the constant A is self-defined by the formula. For $D = 0$, $d = 0$; but for D = infinity, $d = A$. In other words, A is the apparent distance of objects at infinity. Perceived distance d increases with true (physical) distance D; but the curve of d plotted on D, starting with the slope $d/D = 1$, flattens out and becomes asymptotic to the limiting horizontal line, $d = A$.

This limiting value, $d = A$, in the basic formula is given by the apparent distance of astronomical objects; it is also equal to the visual distance from the observer to the perspective horizon (vanishing point for parallel lines receding from the observer).

The O constant or parameter A is a measure of the finite depth of visual space for a given O under given conditions. An O with a higher value of A has an expanded visual world with a wider or farther horizon. An O with a lower value of A has a more compressed or limited visual world with a closer sky and a closer horizon.

The fact that the sky generally appears like a flattened bowl indicates that A has a lower value for pure space perception (elevated vision without foreground) and a higher value for horizontal vision (with foreground), due to differences in clues and context (the "moon illusion").

The high relative compression of visual space into a limiting boundary compared with the corresponding infinite expanse of physical space is impressively illustrated by the following numercial example (calculated for a convenient assumed value of A = 100 ft.):

	Physical Scale (ΔD)		Visual Scale (Δd)
The first	100 ft. of D will be perceived as	50 ft.	$= \frac{1}{2} A$
The next	200 ft. of D will be perceived as	25 ft.	$= \frac{1}{4} A$
The next	400 ft. of D will be perceived as	12.5 ft.	$= \frac{1}{8} A$
The next	800 ft. of D will be perceived as	6.25 ft.	$= \frac{1}{16} A$
The next	1600 ft. of D will be perceived as	3.125 ft.	$= \frac{1}{32} A$

Summation (limit)

For D = infinity, d = 100 ft. = A

Accordingly, distance discrimination ($\Delta d/\Delta D$) rapidly diminishes with distance.

The larger the value of A, the more nearly perceived distance d approximates true distance D. For A = infinity, perceived distance would be equal to true distance.

The plotted functions representing equation (I) for several values of A are shown as a family of curves in Fig. 1. Perceived distance d is plotted against true distance D, in the same units and to the same scale. The diagonal straight line forming the upper limiting boundary represents the function for true or undiminished distance perception. It is obtained from equation (I) with A = infinity. The curves below this boundary line express equation (I) with respective numerical values of A corresponding

to 50, 60, 70, 80, 90, 100, 120, and 200 ft. Larger values of A shift the position of the function upward toward the diagonal line for true distance; smaller values of A shift the function downward toward minimum perception of distance, corresponding to the lack of adequate cues for distance perception.

The basic formula for *perceived size* is

$$\frac{s}{S} = \frac{B}{A + D}, \qquad \text{(II)}$$

where s = perceived size, S = subjective true size (perceived size at normal viewing distance δ,) and $B = A + \delta$. The terms A and D retain the definitions of equation (I). The correction term δ (delta) is the distance at which an object is viewed for subjective true size. This correction makes perceived size equal to true size ($s = S$) not at $D = 0$, but at some small value of $D = \delta$. Thus, an object appears to be true size when it is presented at some normal viewing distance (about 2 feet) in front of the observer. This constant δ may vary with the O, also with different objects for the same O. This device permits us to measure perceived size s (at various distances) and subjective true size S in the same units and to the same scale. The subjective scale by which s and S are measured is not involved and need not be known or expressed. We work only with the ratio s/S. Both are subjective magnitudes, and the scale is immaterial.

Accordingly, the three distance terms (A, B, D) may be given in one set of units, and the two size terms (s and S) in another set of units.

The plotted functions representing equation (II) are shown as a family of curves in Fig. 2. The ratio of perceived size to true size (s/S) is plotted as a function of real distance D. For A = infinity, the upper horizontal boundary line is obtained, representing $s/S = 1$; this relation is size constancy. For $A = 0$, the bottom hy-

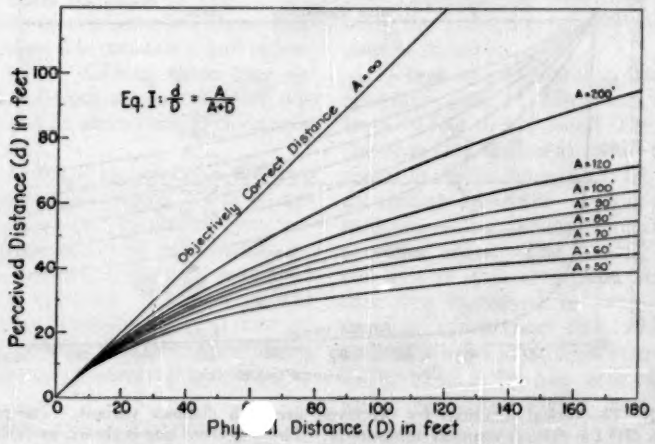

FIG. 1. Theoretical functions for perceived distance. The graphs of equation (I) showing perceived distance plotted against physical distance for various assumed values of A. For $A = \infty$ equation (I) yields the diagonal line representing veridical distance perception. Below this boundary line decreasing values of A displace the curves downward toward minimum distance perception.

perbolic boundary curve is obtained, representing $s/S = \delta/D$; this is the law of retinal image. Both of these special cases or boundary laws are given by equation (II) upon substituting the appropriate values of A (zero or infinity). Intermediate values of A (such as $A = 50$ to 200 feet) yield the intermediate curved graphs shown in Fig. 2.

The single parameter A in both basic formulas thus governs the position of the respective functions between the limiting boundary laws. For perceived distance, the magnitude of A determines the position of the visual distance function between the two extremes of $d = D$ (or perceived distance identical with true distance) and $d = 0$ (or greatly diminished distance perception). In similar manner, the magnitude of A governs the position of the visual size function between the two boundary conditions representing size constancy and the opposite extreme of perceived size proportional to and dependent only upon retinal image or visual angle.

Accordingly, the magnitude of A is a simple, convenient, and direct measure or index of what has been termed by prior investigators "phenomenal regression to the real object" (16). A zero value of A represents zero "regression," and an infinite value of A represents complete or one hundred per cent "regression." An advantage of such use of A over other indices of "regression" is that it is a constant for a given O under given conditions over the entire range of an experiment, whereas other indices vary from observation to observation or from point to point of a plotted experimental curve.

The empirical value of A (for a given O under given conditions) may be determined by any two or more

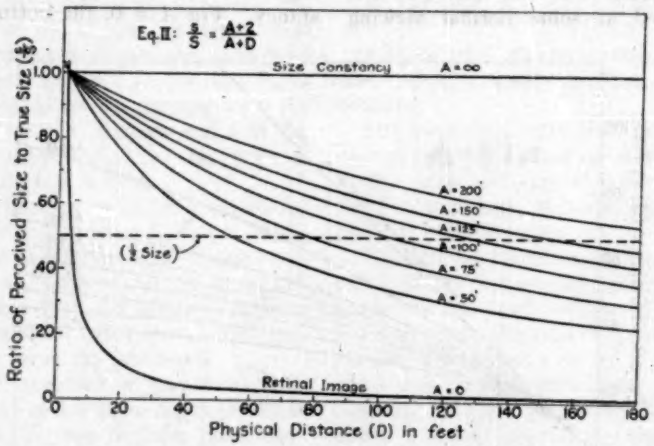

FIG. 2. Theoretical functions for perceived size with distance variant. The graphs of equation (II) for various assumed values of A. The perceived size is shown as ratios to the true size (perceived size of the stimulus object at a normal viewing distance, taken as 2 feet). The value of A governs the position of the functions between and inclusive of the horizontal solid line representing the law of size constancy and the bottom curve representing the law of the retinal image or visual angle. The horizontal dashed line cuts the curves at $s = \frac{1}{2}S$, indicating the distance at which this ratio may be expected for different values of A.

observations at different distances, or by a single observation of an astronomical object. (See Appendix IV.)

Instead of concentrating upon the effect of varying the parameter A, we may also study equation (II) with reference to the effect of varying the distance D. For $D = \delta$, $s = S$, and the object is seen as "true size." As the object is brought closer to the eye, perceived size s increases somewhat; and when the object is moved away from the eye, perceived size diminishes. (Luneburg's derived formulas yield the anomalous result of making perceived size diminish steeply when the object is brought close to the eye. See his Fig. 41.)

In further reference to equation (II), for small values of the distance D (relative to A), s/S is close to unity, corresponding to the law of size conconstancy; and for very large distances D (relative to A), s/S is greatly diminished and varies inversely with D, corresponding to the law of retinal image.

The basic formulas (I and II) are thus seen to express a law intermediate between size constancy and retinal image while yielding these two extreme conditions at short viewing distances and at astronomical distances, respectively.

In the following sections the two basic formulas (I and II) will be applied to recorded results of prior experiments and to new experiments involving observations of perceived size and distance. These data will provide an initial empirical test of the validity and practical usefulness of the present theoretical formulation.

EXPERIMENTAL CONFIRMATION

Perceived Size. A number of previously recorded experiments, in which the subject adjusts the size of a comparison stimulus at a constant distance to match a standard stimulus at a variable distance, yield results which correspond neither to the law of retinal image nor to the law of size constancy.

Instead, beginning with the first systematic experiment on perceived size (Martius, 1889), the size matches consistently follow an *intermediate law* (**2, 8, 9, 11, 16**). The dependence of perceived size upon distance is found to be in good agreement with the prediction expressed by equation (II) for perceived size. The slope of the function relating the adjusted size of the comparison object and the distance of the standard object is affected by controlling circumstances (**4, 8, 9, 18**). These variations are reflected by appropriate changes in the magnitude of the parameter A in equation (II).

The most adequate investigation of this problem was performed by Holway and Boring (**9**). Their experiment measured perceived size at distances up to 120 feet, and also showed how the resulting functions shifted with the reduction of the available cues to distance.

O's task was to match a controlled standard disk at distances varying from 10 feet to 120 feet. The size of the standard disk was made proportional to the distance so that it always subtended an angle of one degree, thus presenting constant retinal image or visual angle. The varying apparent size of this controlled standard disk was measured by means of a variable comparison disk which remained always at 10 feet from *O*.

The results for two sets of conditions, binocular regard and direct monocular regard, for Boring himself as *O*, are shown in Fig. 3. Binocular vision gives a function close to the ascending straight-line function corresponding to size constancy. Re-

duction to monocular vision lowered the function nearer to the horizontal slope corresponding to perceived size dependent only upon retinal image. Both of the curves drawn through the data points are given by equation (II) for perceived size.

For the upper curve, obtained for binocular vision, the parameter A is 243 feet; for the lower curve, obtained for monocular vision, the parameter A is 132 feet. The value of A is reduced when the perceptive cues are reduced. For both equations, B is conveniently taken as equal to A plus 10 feet, 10 feet being the distance of the comparison object. The same formula (equation II) applies to both binocular and monocular vision, with only a change in the parameter A.

From these results we learn that the value of A is, in fact, governed by the available cues to distance. The value of A determines the position of the intermediate function (expressed by equation II) between the two extremes, size constancy and retinal image. The constant A is thus demonstrated to be a sensitive index or measure of "phenomenal regression" and of the conditions which affect its attainment.

Perceived Distance by Equal-Apparent Intervals. In connection with this study two pilot experiments were performed, dealing with the relation of perceived distance d to physical distance D. Both experiments were designed to serve as an initial test of equation (I) for perceived distance, and as a guide to more refined experimentation.

Experiment I used a modified method of equal appearing intervals. O stood at one end of an unfamiliar indoor archery range, about 80 feet

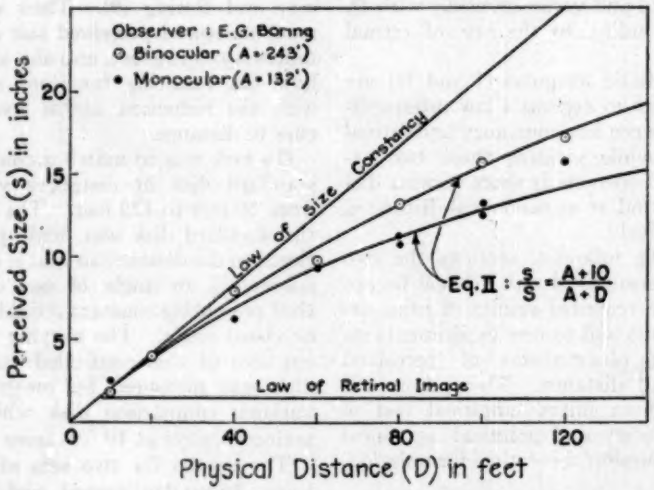

FIG. 3. Perceived size as a function of distance for binocular and monocular observation. The data are from Holway and Boring (9) with their own correction for space error. Equation (II) fits both sets of observations with only a change in the parameter A. As the conditions are altered from direct monocular regard to direct binocular regard, the parameter A has a higher value, shifting the function upward to approach the top diagonal line for size constancy. This line rises because they increased the size of the standard in proportion to the distance to keep visual angle constant.

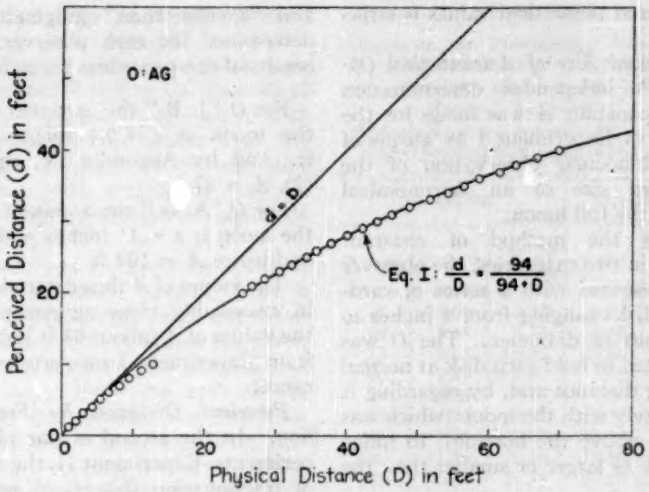

FIG. 4. Perceived distance as a function of physical distance (Exp. I). The circles represent successive equal apparent increments of distance from 0 to 10 subjective feet, and from 20 to 40 subjective feet. At 10 subjective feet the *O* was told to indicate the distance that appeared twice as great, thus determining the physical magnitude to be assigned to 20 subjective feet. The data lie on the curve of equation (I) with $A = 94$ feet. The diagonal line represents veridical distance perception.

ong, and directed the experimenter, who moved a pointer stick at a slow and nearly constant rate along the ground away from *O*, to mark off successive increments of equal perceived length. Each successive increment of perceived distance was an attempt to match, by non-simultaneous viewing, a memorized "subjective foot rule" in the case of one observer and a memorized "subjective meter stick" in the other. Successive division points were temporarily marked by a horizontal rod marker which was moved as soon as each next step had been attained.

The results for the two *O*s are shown in Figs. 4 and 5. In order to illustrate the manner in which the data are analyzed, Table I presents for one observer the cumulative measured distance D in physical units for each value of subjective distance d; the calculated value of A for each separate observation ($A = Dd/(D-d)$, Appendix IV, equation 5); and the theoretical value of D from equation (I), using the weighted mean value of A determined from these data. It is significant to note the experimental constancy of A over the series of observations, particularly toward the larger values of D; also the correspondence between the measured and the calculated values of D. (For smaller distances, small experimental deviations are reflected by larger variations in the calculated value of A.)

In Figs. 4 and 5, perceived distance d is plotted against physical distance D in the same units and to the same scale. The diagonal straight line is the theoretical function for perceived distance identical with true measured distance. The obtained points lie on the tangent curves given by equation (I). The agreement between ob-

tained and theoretical values is striking.

Perceived Size of Astronomical Objects. An independent determination of the constant A was made for the two *O*s of Experiment I by means of direct binocular observation of the perceived size of an astronomical object, the full moon.

Using the method of constant stimuli in two categories, the observer was presented with a series of cardboard disks ranging from 4 inches to 30 inches in diameter. The *O* was instructed tc hold each disk at normal viewing distance and, by regarding it alternately with the moon (which was slightly above the horizon), to judge the disk as larger or smaller than the moon.

Ten judgments by the *O* were obtained for each disk within the transition zone, and the stimulus which received an equal number of "larger" and "smaller than" judgments was determined for each observer. The results of this procedure are as follows:

For *O* "J. B." the apparent size of the moon is $s = 9.4$ inches $= 0.78$ ft.; and by Appendix IV, equation (2), $A = 87$ ft.

For *O* "A. G." the apparent size of the moon is $s = 11$ inches $= 0.92$ ft., and hence $A = 102$ ft.

The values of A thus determined are in reasonably close agreement with the values of A (about 94 ft.) obtained from Experiment I on perceived distance.

Perceived Distances by Fractionation. In the second of our pilot experiments, Experiment II, the method of fractionation (bisection) was employed in order to construct a ratio scale of visual distance and to determine the relation of visual distance to the stimulus magnitude over a greater

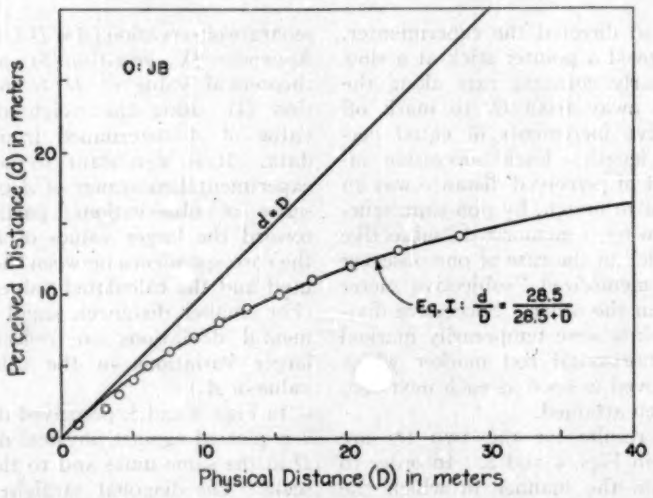

FIG. 5. Perceived distance as a function of physical distance (Experiment I). The circles represent successive equal apparent increments of distance from *O* to 14 subjective meters. The meter was preferred by this *O* as the more familiar subjective measuring rod. The data (also shown in Table I) lie on the curve of equation (I) with $A = 28.5$ meters (about 94 feet) and agree closely with the results of the other *O* of Experiment I.

range than was previously investigated. It was thought that this method would provide a more logically defensible test of the theoretical function (equation I) and would also allow a direct comparison with the procedure adopted by Stevens and Volkmann (**15**), and Reese (**14**) for constructing a subjective magnitude function.

O's task was to bisect each one of fourteen distances, ranging from 8 feet to 200 feet, on a large flat lawn. A line was stretched across the lawn, perpendicular to *O* who stood a few inches away from the end of the anchored line so that she could see all of the line. Bisections were performed by stopping a pointer, which moved back and forth along the line, at a point which appeared to be halfway between the near end of the line and a marker designating the total distance to be bisected. Four sets of half-distance observations were obtained, the standard distances being presented in serial order, alternately ascending and descending. Each bisection was marked with a golf tee, invisible to the observer, and the judged half-distances were measured and the tees removed between series. Two naive *O*s took part in the experiment.

TABLE I

OBSERVED AND THEORETICAL VALUES OF PHYSICAL DISTANCE AND THE CALCULATED VALUE OF A FOR EACH OBSERVATION OF PERCEIVED DISTANCE

(Data from *O*: J. B., Exp. I)

Perceived d (meters)	Observed Physical D (meters)	Calculated $A = Dd/(D-d)$		Eq. I, with $A = 28.5$
		A (meters)		Calc. D (meters)
1	1.08	14		1.04
2	3.09	5		2.15
3	4.12	11		3.35
4	5.12	18		4.65
5	6.10	28		6.05
6	7.59	29		7.60
7	9.39	27		9.28
8	11.08	29		11.10
9	13.10	29		13.14
10	15.34	29		15.40
11	17.60	29		17.91
12	20.37	29		20.74
13	23.56	29		23.94
14	27.96	28		27.52

Weighted Mean $A = 28.5$

TABLE II

OBSERVED AND THEORETICAL VALUES OF HALF DISTANCE (D_1) FOR EACH STANDARD (D_2) FOR THE TWO *O*s OF EXPERIMENT II

(Calculations from equation (2) using the respective weighted mean values of A)

Observer BS			Observer DM	
D_2 (feet)	D_1 (feet)	Calc. D_1 (feet) ($A = 180$)	D_1 (feet)	Calc. D_1 (feet) ($A = 200$)
8	4.00	3.91	3.67	3.92
10	5.15	4.86	4.58	4.88
12	6.63	5.81	5.00	5.82
16	7.89	7.67	7.83	7.69
20	10.06	9.46	8.50	9.54
30	13.67	13.87	11.67	13.93
40	17.25	18.00	14.83	18.18
60	21.59	25.74	19.58	26.05
80	27.33	32.75	29.58	33.33
100	34.71	39.10	35.50	40.00
120	41.15	45.00	48.08	46.15
150	51.98	52.90	54.25	54.50
180	61.52	60.00	66.00	62.05
200	64.54	64.15	70.00	66.67

The results of this procedure (for each *O*) are shown in Table II, and the half-judgment function (for the combined data) is plotted as illustrated in Fig. 6. From such half-judgment plots, subjective magnitude functions may be constructed according to the somewhat cumbersome procedure described in detail by Reese (**14**). For the present problem, the identical functions may be obtained by a much simpler method, by utiliz-

ing a convenient property of the basic equation (I).

From equation (I), the half-judgment function must satisfy the relation

$$D_1 = \frac{(\frac{1}{2} D_2) A}{A + (\frac{1}{2} D_2)}. \qquad (2)$$

Equation (I) may also be written in the form

$$d = \frac{D A}{A + D}. \qquad (3)$$

These two functions, equations (2) and (3), are obviously identical. The relation of D_1 to $\frac{1}{2}D_2$ is identical with the relation of d to D. The first is the half-magnitude function, and the second is the subjective magnitude function. This simple relation may be used to obtain a d curve from a D_1 curve or a D_1 curve from a d curve. By simply using a double horizontal scale, a single plotted curve thus serves a dual purpose.

Figure 7 graphically shows this convenient identifying relation between the subjective scale of distance d and the half-judgment curve D_1 replotted from Fig. 6. D_1 is the same curve as d but with the horizontal scale doubled. In Fig. 7, locate the ordinate $d = D_1 = 22$ feet. The distance $D_1 = 22$ from the D_1 curve was judged to be half of $D_2 = 50$ feet. Halving this abscissa yields $D = 25$ feet and, consequently, for $d = 22$, $D = 25$. Accordingly, the experimental curve of Fig. 6 becomes directly a subjective magnitude function by simply using the upper horizontal scale D instead of the lower D_2 scale, and by reading the ordinate as d in place of D_1.

The points marked by triangles on the subjective magnitude function of Fig. 7 are determined by the conventional method (14) by arbitrarily assigning one subjective foot to one physical foot and then proceeding by interpolation of the half-judgment function to discover the successive physical magnitudes to which the suc-

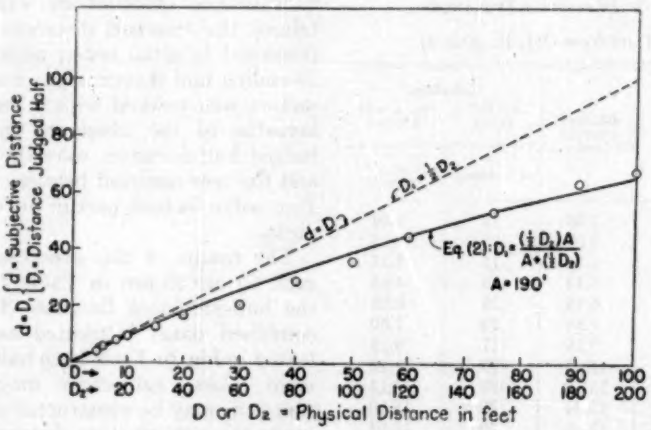
d = Subjective Distance
D1 = Distance Judged Half
d = D1
D1 = ½ D2
d = D
Eq (2): D1 = (½ D2)A / A + (½ D2)
A = 190'
0
20
40
60
80
100
D → 10 20 30 40 50 60 70 80 90 100
D2 → 20 40 60 80 100 120 140 160 180 200
D or D2 = Physical Distance in feet

FIG. 6. The mean bisection data of Experiment II are plotted on the curve of equation (2) in the text showing the half-judgment function, or, using the upper horizontal scale (D) and reading d in place of D_1 on the vertical scale, the subjective scale of visual distance. The inclined dashed line shows the objective half distance and objective full distance for the lower and upper horizontal scales, respectively. See text and Fig. 7 for explanation.

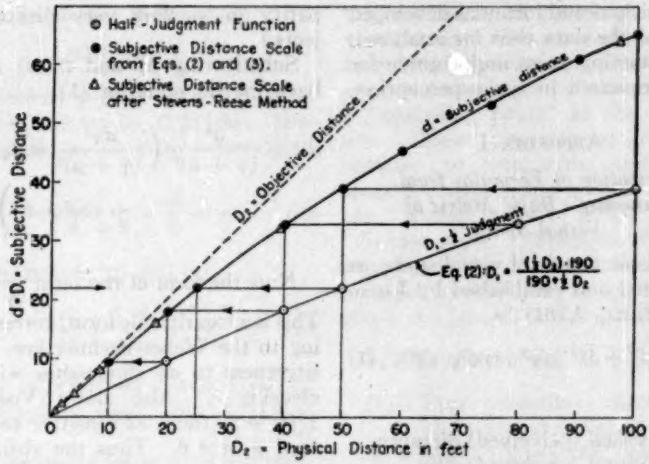

FIG. 7. The magnitude function for subjective distance erected from the half-judgment function by two different procedures. The construction lines show how the magnitude function is obtained from the curve fitted to the mean bisection data by simply halving the horizontal scale, thus utilizing a convenient property of the present formulation. The identical function is obtained by the conventional method (14); see triangles.

cessively doubled numerals (2, 4, 8, 16, 32, 64) should be assigned. This method yields only seven points from these data, whereas the present more expeditious and direct method allows us to use all of the observed points and hence produces a more reliable curve. The function obtained by both methods is identical. The validity of the basic equation (I) is thus doubly confirmed.

SUMMARY

A general unifying law of visual space perception is shown to be capable of rational derivation and quantitative formulation. The derived basic formulas for perceived size and distance as functions of true size and distance are tested and confirmed by application to recorded data of antecedent experiments and to new observations of perceived size and distance. The excellent agreement between observed and calculated values indicates that these formulas are valid not only for the primary case of pure space perception, but also for the more general case of visual space perception (binocular or monocular) with any enrichment or diminution of visual and related cues.

The concept of a maximum limit of perceived distance yields a single parameter, A, in the basic formulas whose magnitude governs the intermediate position of the functions between the boundary conditions expressed by size constancy and retinal image, respectively. The numerical value of this parameter, A, for a given observer is shown to depend upon the available perceptual cues to distance, and, hence, provides a convenient direct index or measure of "phenomenal regression."

The next indicated study is a planned series of experiments to determine the conditions and laws of variation of the parameter A. The work-

ing definitions and formulas developed here, and the data thus far analyzed, offer a starting point and a guide for further research in space perception.

APPENDIX I

Derivation of Formulas from Luneburg's Basic Metric of Visual Space

The basic metric of visual space, as formulated and established by Luneburg (12, Eq. 3.781), is

$$ds^2 = \overline{d\ (d)}^2 + M^2\ (d\varphi^2 + \cos^2\varphi \cdot d\theta^2), \quad (1)$$

where

d = visual (perceived) distance,
s = visual (perceived) size,
φ = horizontal angle (azimuth),
θ = vertical angle (altitude),
M = linear size factor = $ds/d\varphi$.

Luneburg makes this basic metric his point of departure for transformations into elliptic, Euclidean, and hyperbolic space geometries, respectively. With the same point of departure, we shall make our Euclidean transformation with the following consistent substitutions:

$$M = d. \quad \text{(Hence } ds = d \cdot d\varphi.\text{)}$$

True (objective) distance = $D = a/\gamma$, where

a = interpupillary distance,
γ = angle of binocular convergence.

$$\text{Perceived distance} = d = \frac{a}{\gamma + \mu}.$$

Hence

$$d\ (d) = -\frac{a \cdot d\gamma}{(\gamma + \mu)^2}.$$

(μ (mu) = a constant added to γ. It limits maximum perceived d, keeping d finite when γ is zero or imperceptibly small. In other words, μ = the minimum or threshold value of *perceived* convergence or horizontal disparity in viewing very distant objects.)

Substituting M and d (d) in the basic metric, equation (1),

$$ds^2 = \frac{a^2}{(\gamma + \mu)^2} \cdot \left(\frac{d\gamma^2}{(\gamma + \mu)^2} + d\varphi^2 + \cos^2\varphi \cdot d\theta^2 \right). \quad (2)$$

Note the form of the term $\frac{d\gamma}{(\gamma + \mu)}$. This is a logarithmic form, corresponding to the Weber-Fechner law. Sensitiveness to $d\gamma$ diminishes with increasing γ. Also note: Vision is $1/(\gamma + \mu)$ times as sensitive to $d\gamma$ as to $d\ \varphi$ or $d\ \theta$. Thus the visual distance factor is $1/(\gamma + \mu)$ times the visual size factor.

The transformed metric, equation (2), contains two constants $(a,\ \mu)$ varying with the observer.

By equation (2), with $d\ \varphi = 0$ and $d\ \theta = 0$,

$$d = a/(\gamma + \mu).$$

But

$$D = a/\gamma.$$

Hence,

$$\frac{d}{D} = \frac{\gamma}{(\gamma+\mu)} = \frac{a/\mu}{a/\mu + a/\gamma} = \frac{A}{A+D},$$

with

$$A = a/\mu.$$

(Two observer constants are thus replaced by one.)

Similarly, by equation (2) with $d\gamma = 0$ and $d\theta = 0$,

$$ds = M \cdot d\varphi = d \cdot d\varphi, \text{ or } s = d \cdot \varphi.$$

But objective true size S_0 (to the same scale as D) is given by

$$dS_0 = D \cdot d\varphi, \text{ or } S_0 = D \cdot \varphi.$$

Hence

$$\frac{s}{S_0} = \frac{d}{D} = \frac{A}{A + D}. \quad (3)$$

Let σ (sigma) $= A/B$, where

$$B = A + \delta,$$

and δ (delta) $= D$ at normal viewing distance. Then, by definition, subjective true size S is given by equation (3) as

$$\frac{S}{S_0} = \frac{A}{A + \delta} = \frac{A}{B} = \sigma. \quad (4)$$

From equations (3) and (4),

$$\frac{s}{S} = \frac{B}{A} \cdot \frac{d}{D} = \frac{1}{\sigma} \cdot \frac{d}{D} = \frac{B}{A + D}. \quad (5)$$

Note: d is perceived distance, to the same scale as visual size s. ($\varphi = s/d$.)

D is measured (objective) distance, to the same scale as measured (objective) size S_0. ($\varphi = S_0/D$.)

A, B, D, and d are given in the same units.

S_0, S, and s are given in the same units.

We have thus derived two simple formulas, for perceived distance and perceived size, respectively:

$$\frac{d}{D} = \frac{A}{A + D}, \quad (I)$$

$$\frac{s}{S} = \frac{B}{A + D}. \quad (II)$$

APPENDIX II

Derivation of Formulas from Principles of Perspective

The basic geometry of vision is represented in Fig. A1, and the basic geometry of perspective is shown in Fig. A2.

Referring to Fig. A2, the compression of distance in perspective corresponds to the compression of perceived *distance* in visual space; and the perspective reduction of size (width or height) between parallel lines receding from the observer corresponds to the reduction of perceived *size* in visual space. The parallel horizontal lines receding from the observer are seen in perspective as converging to a "vanishing point" in the horizon line. These proportional relations common to perspective and visual space are shown in Fig. A2. (VP is the "vanishing point"; DP is the "division point." From the objective plan below the ground line, the geometrical perspective is constructed above the ground line as shown.)

In Figs. A1 and A2,

D = True (objective) distance; d = Perceived distance.

S_0 = True (objective) size; s = Perceived size.

A = Horizon distance = Apparent distance of vanishing points in the horizon.

By Fig. A1, the visual angle φ at the nodal point of the eye is given by

$$\varphi = \frac{s}{d} = \frac{S_0}{D}.$$

Hence,

$$\frac{s}{S_0} = \frac{d}{D}. \quad (1)$$

By similar triangles in Fig. A2,

$$\frac{s}{S_0} = \frac{A - d}{A}. \quad (2)$$

From equations (1) and (2),

$$\frac{d}{D} = \frac{A - d}{A}. \quad (3)$$

This reduces to

$$\frac{d}{D} = \frac{A}{A + D}. \quad (I)$$

Hence, by equation (1),

$$\frac{s}{S_0} = \frac{A}{A + D}. \quad (4)$$

By definition of the proportionality constant σ (sigma),

$$\sigma = \frac{S}{S_0} = \frac{A}{B}. \qquad (5)$$

From equations (4) and (5),

$$\frac{s}{S} = \frac{B}{A + D}. \qquad (II)$$

The two basic formulas (I) and (II), thus written directly from the geometry of perspective, are identical with the two basic formulas derived rigorously in Appendix I from Luneburg's more theoretical metric of visual space.

APPENDIX III

Derivation of Basic Formula by Composition of the Laws of Size Constancy and Visual Angle

It is known that the law of perceived size is intermediate between the law of retinal image (visual angle) and the law of size constancy (**2, 9**). This intermediate relationship has been called "regression to the real object" (**16**).

The law of retinal image is expressed by

$$\frac{s}{S} = \frac{\delta}{D}, \qquad (1)$$

where δ (delta) is the distance at which an object appears "true size" ($s = S$). This would make the perceived size s of an object vary inversely with its distance D.

The law of size constancy is expressed by

$$\frac{s}{S} = \frac{A}{A}. \qquad (2)$$

This would make $s = S$, independent of distance.

By composition of equations (1) and (2), combining the two extreme laws into a single generalized intermediate law, we have

$$\frac{s}{S} = \frac{A + \delta}{A + D}. \qquad (II)$$

The value of A (or the ratio of $A:D$) determines the relative position of the interpolated law (II) between the two extremes, (1) and (2). Compare the Thouless (**16**) coefficient. For $A = 0$, equation (II) becomes the law of retinal image. For $A =$ infinity, equation (II) becomes the law of size constancy. For $A = D$(or $D = A$), equation (II) gives a value exactly midway between the two limiting laws, (1) and (2).

The magnitude of A (between zero and infinity) is thus a measure of the

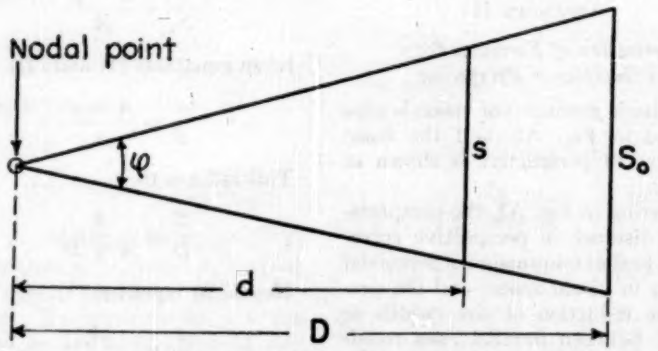

FIG. A1. Geometry of visual space.

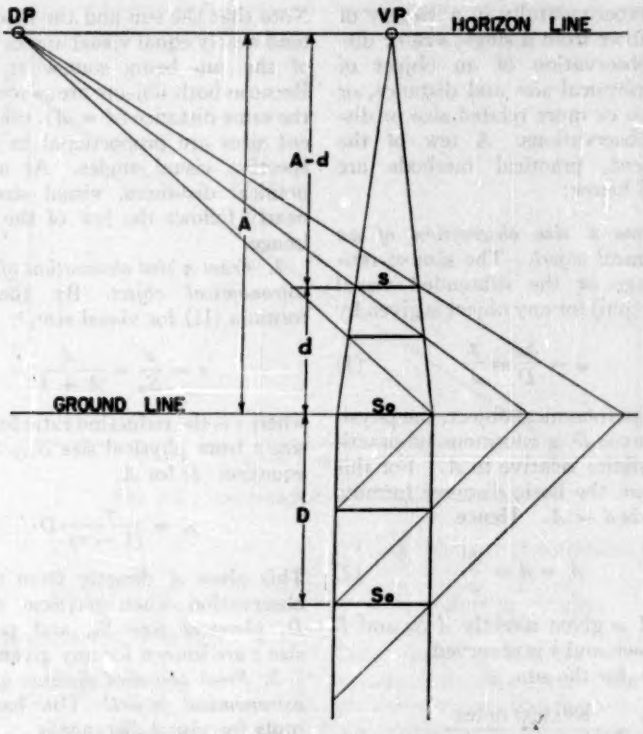

FIG. A2. Perspective geometry.

"regression to the real object." For $A = 0$, the regression is zero. For A = infinity, the regression is complete. For $A = D$, the departure is half-way. The difference between retinal image and size constancy is divided into two parts by the perceived size; the ratio of the two parts is $A:D$.

At small values of D (compared to A), the perceived size, by equation (II), is close to size constancy; the difference is so small as to be missed by investigators.

At very large values of D (compared to A), the perceived size, by equation (II), is close to the retinal image law. The sun and the moon appear nearly the same size because their retinal images are nearly the same size, although their real sizes are 880,000 miles and 2160 miles, respectively, a ratio of 407 to 1.

Equation (II) is the basic formula for visually perceived size, identical with the basic formula derived in the preceding Appendices. It is a generalized, unifying expression, embracing the laws of size constancy and retinal image as special or limiting cases and bridging the gap between those two extreme laws.

APPENDIX IV

Experimental Determination of the Visual Constant A

The visual constant A for a given O under given conditions may be deter-

mined experimentally in a variety of ways, either from a single size or distance observation of an object of known physical size and distance, or from two or more related size or distance observations. A few of the convenient, practical methods are outlined below:

1. *From a size observation of an astronomical object.* The size of retinal image or the subtended visual angle φ (phi) for any object is given by

$$\varphi = \frac{S_0}{D} = \frac{s}{d}. \qquad (1)$$

For an astronomical object, the physical distance D is enormous or practically infinite relative to A. For this condition, the basic distance formula (I) yields $d = A$. Hence

$$A = d = \frac{s}{\varphi}. \qquad (2)$$

Thus A is given directly if S_0 and D are known and s is observed.

Thus, for the sun,

$$\varphi = \frac{S_0}{D} = \frac{880{,}000 \text{ miles}}{92{,}000{,}000 \text{ miles}} = 0.0096 \text{ radians.}$$

If the apparent size of the sun is

$$s = 15 \text{ in.} = 1.25 \text{ ft.},$$

then

$$A = d = \frac{s}{\varphi} = \frac{1.25 \text{ ft.}}{0.0096} = 130 \text{ ft.}$$

Similarly, for the moon,

$$\varphi = \frac{S_0}{D} = \frac{2160 \text{ miles}}{244{,}000 \text{ miles}} = 0.0090 \text{ radians.}$$

If the apparent size of the moon is

$$s = 14 \text{ in.} = 1.17 \text{ ft.},$$

then

$$A = d = \frac{s}{\varphi} = \frac{1.17 \text{ ft.}}{0.0090} = 130 \text{ ft.}$$

Note that the sun and the moon subtend nearly equal visual angles φ, that of the sun being somewhat larger. Because both objects are perceived at the same distance ($d = A$), the apparent sizes are proportional to the respective visual angles. At astronomical distances, visual size more nearly follows the law of the retinal image.

2. *From a size observation of a non-astronomical object.* By the basic formula (II) for visual size,

$$r = \frac{s}{S_0} = \frac{A}{A + D}, \qquad (3)$$

where r is the reduction ratio for visual size s from physical size S_0. Solving equation (3) for A,

$$A = \frac{r}{(1 - r)} \cdot D. \qquad (4)$$

This gives A directly from a single observation when physical distance D, physical size S_0, and perceived size s are known for any given object.

3. *From perceived distance of a non-astronomical object.* The basic formula for visual distance is

$$\frac{d}{D} = \frac{A}{A + D}. \qquad (I)$$

Solving for A,

$$A = \frac{D \cdot d}{D - d}. \qquad (5)$$

This gives A directly from a single distance-observation for any object, when physical distance D and visual distance d are given.

4. *From two size-observations of the same object.* Let s_1 be the perceived size of a given object at measured distance D_1, and s_2 the perceived size of the same object at measured distance D_2. By the basic formula (II) for visual size,

$$\frac{s_1}{S} = \frac{B}{A + D_1} \qquad (6a)$$

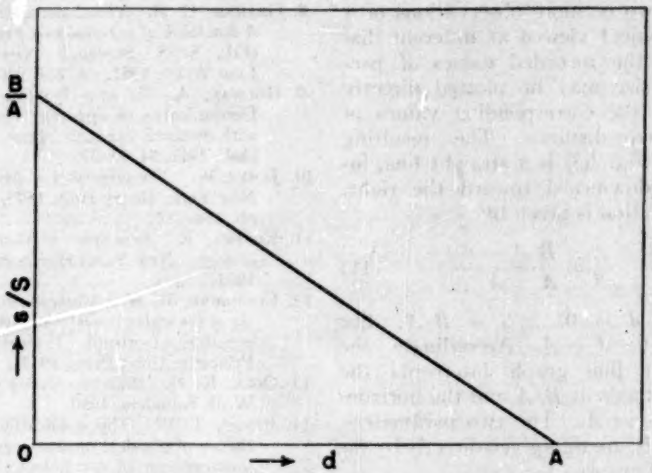

FIG. A3. Perceived size and perceived distance.

and

$$\frac{s_2}{S} = \frac{B}{A + D_2} \tag{6b}$$

From equations (6a) and (6b),

$$\frac{s_1}{s_2} = \frac{A + D_2}{A + D_1}. \tag{7}$$

Solving equation (7) for A,

$$A = \frac{D_2 s_2 - D_1 s_1}{s_1 - s_2}. \tag{8}$$

This gives A directly when the two physical distances, D_1 and D_2, and the two respective perceived sizes, s_1 and s_2, of a given object are known.

5. *From a half-distance observation.* Let D_1 be the measured distance of the apparent (visually perceived) midpoint of the measured distance D_2. By the basic formula (I), the half-distance observation is expressed by the pair of equations:

$$d_1 = \frac{A \cdot D_1}{A + D_1} \tag{9a}$$

and

$$d_2 = 2d_1 = \frac{A \cdot D_2}{A + D_2}. \tag{9b}$$

Solving this pair of equations for A

$$A = \frac{D_1 \cdot D_2}{D_2 - 2D_1}. \tag{10}$$

This gives A directly when any measured distance D_2 and the apparent half-distance D_1 are given.

6. *From a series of observations in a visual size or distance experiment.* As a rule, a larger number of observations make up a size or distance experiment. In such case, the best mean value of A is obtained as a weighted mean of the values of A computed for the individual observations (using the respective distances D as the weight factors). Other convenient methods of curve-fitting or short-cut applications of the Theory of Least Squares may be used to determine the best mean value of A to fit the series of observations.

After A is determined, B is given by $B = A + \delta$, where δ (delta) is the normal viewing distance for the observer, usually 1 or 2 ft.

7. *From coupled observations of perceived size and perceived distance.*

From two or more observations of a given object viewed at different distances, the recorded values of perceived size may be plotted directly against the corresponding values of perceived distance. The resulting graph (Fig. A3) is a straight line, inclined downward toward the right. The relation is given by

$$\frac{s}{S} = \frac{B}{A} \cdot \frac{A - d}{A}. \qquad (11)$$

For $d = 0$, $s/S = B/A$. For $s/S = 0$, $d = A$. Accordingly the straight line graph intercepts the vertical axis at B/A and the horizontal axis at A. The two parameters, A and B, are thus given directly by the plotted graph.

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